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A
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ILLUSTRATED WITH ENGRAVINGS.

BY WILLIAM NICHOLSON.

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THE ARTS.

APRIL 1801.

ARTICLE I.

Some Observations on different Hydrocarbonates and Combinations of Carbone with Oxygen, &c. in reply to some of Dr. Priestley's late Objections to the New System of Chemistry. By W. CRUICKSHANK, Woolwich.

ON perusing Dr. Priestley's late publication, in which he supports the doctrine of phlogiston, I was much struck with some of the experiments there related, more especially with the quantity and nature of the gases obtained from the grey oxide of iron and charcoal, and the same oxide with the carbonate of barytes.

Although I had not the least doubt of the accuracy of these experiments, yet I was determined to repeat, and extend them, and to pay particular attention to the nature of the different gases obtained.

Accordingly a quantity of the grey oxide, or scales of iron separated in forging, were introduced into a crucible, and exposed to a red heat for upwards of half an hour; at the same time a quantity of powdered charcoal was introduced into another crucible, which had a cover luted on, and likewise exposed to a strong red heat; these were mixed while hot, and introduced into a small coated glass retort. The retort was then placed in a furnace, its neck being plunged under water, in a pneumato-chemical tub, and heat gradually applied.

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By the time that the retort became red, or rather sooner, gas was produced in great abundance, and continued to come over for two hours or upwards; this gas was received in different vessels, so that the portions obtained at different periods of the process might be examined separately: the whole amounted to 150 oz. measures (troy.)

The first portion consisted of carbonic acid gas, one part, and inflammable gas four parts.

The second portion of carbonic acid gas, one part, and inflammable gas five parts.

The third was exactly similar to the second, and then to the end of the process; it consisted pretty uniformly of carbonic acid gas, one part, and inflammable gas, six parts. I repeated this experiment, and employed an iron retort, in order that a much greater degree of heat might be applied. In this case the gases obtained were exactly similar, and the very last portion was found to consist of carbonic acid gas one part, and inflammable gas six parts; but the quantity in all, although the oxide of iron and charcoal together did not exceed two ounces, was prodigious, and must have amounted to many gallons, by measure.

Suspecting that some of the other metallic oxides might yield similar results, if treated with charcoal in the same manner; I exposed a quantity of the sublimed oxide of zinc to a red heat for nearly an hour, and then mixed it with charcoal, which had likewise been previously heated; these were introduced into a coated glass retort, and subjected to a heat gradually raised as in the former experiments; before the retort became red, a considerable quantity of gas was disengaged, but upon increasing the heat it came over in torrents.

The first portion consisted of carbonic acid gas, and inflammable gas, in the proportion of 1 to 9. The second, of the same gases, in the proportion of 1 of the former, to 26 of the latter, and what was received afterwards, was purely inflammable; the whole amounted to 90 ounce measures. At the end of the process a quantity of metallic zinc was found in the neck of the retort.

I next subjected the red oxide of copper and charcoal to a similar process, and the quantity of gas collected from an ounce of this mixture, amounted to 64 ounce measures.

The first portion consisted of the carbonic acid, and inflammable gases, in the proportion of 10 of the former to one of the latter. The second of the same gases, in the proportion of 15 to 55; and the third was purely inflammable gas, without any sensible quantity of carbonic acid. At the end of the process the copper was found in small globules in its metallic state, mixed with the charcoal.

The next subject of experiment was litharge, which was dried as much as possible, mixed with hot charcoal, then introduced into a coated glass retort, and exposed to heat in the same manner as the other mixtures.

The first portion of gas consisted of carbonic acid, and inflammable gas very nearly in equal bulks. The second, of the same gases in the proportion of 13 of the former to 39 of the latter, and what came over afterwards was almost purely inflammable, the whole amounting to about 40 ounces. The lead was found revived and mixed with the charcoal in the form of small globules.

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The last metallic oxide which I subjected to this process, was that of manganese; about half an ounce of the black ore of this semi-metal was exposed for an hour to a strong red heat in a crucible, it was then mixed with hot charcoal, and introduced into a coated glass retort; heat being applied and gradually increased, a quantity of gas, amounting in all to 38 ounces, was procured. The first portion was chiefly carbonic acid; the second a mixture of this with inflammable gas, in nearly equal proportions; and the last almost purely inflammable.

From these experiments we may draw the following conclusions: 1st. That all metallic oxides capable of enduring a red heat, will, when mixed with charcoal, not only yield carbonic acid, but also a very considerable quantity of an inflammable gas. 2dly, That those oxides which retain their oxygen most obstinately, yield the greatest quantity of inflammable gas; and on the contrary, that those which part with it readily, afford the greatest proportion of carbonic acid. And 3dly, That the carbonic acid gas was chiefly disengaged at the beginning of the process, and the purest and greatest quantity of inflammable gas towards the conclusion of it.

Having thus ascertained the relative proportions of these gases, I next endeavoured to investigate the properties of the inflammable gas, which from the peculiar circumstances under which it was procured, I had little doubt must be different from any of the known hydrocarbonates.

Accordingly I took a quantity of that obtained from the grey oxide of iron, and washed it repeatedly with lime water, until the whole of the carbonic acid was completely separated. Its specific gravity was then ascertained, and it was found to be but a very little lighter than common air, that is to say, in the proportion of about 22 to 23, a circumstance which proved it at once to be different from the common hydrocarbonates, all of which are much lighter than common air.

I next endeavoured to estimate the quantity of pure oxygen necessary to saturate it, and likewise the products of the combustion. After several trials I found, that when four measures of the washed gas, mixed with two of oxygen, were introduced into a strong glass jar over mercury, and fired by the electric spark, they were reduced to $3\frac{1}{2}$ measures, and that when this residuum was agitated with lime water, there only remained about $\frac{1}{4}$ of a measure, which was found by the nitrous test to be pure oxygen; hence it would appear, that eight measures of this gas require $3\frac{1}{2}$ measures of pure oxygen gas to saturate them, and when combined produce nearly six measures of carbonic acid gas, with a little water. Now if we suppose a measure equal to a cubic inch, and that a cubic inch of the inflammable gas, at the temperature of 55° , and under a mean pressure of the atmosphere, weighs .3 of a grain, a cubic inch of oxygen gas, .34 of a grain, and a cubic inch of carbonic acid gas .47 of a grain; then the weight of eight measures of the inflammable, with $3\frac{1}{2}$ of oxygen gas, will be 3.6 grains very nearly; but the weight of the carbonic acid gas generated, is only 2.8 grains; hence there remains a deficiency of .8 of a grain to be accounted for, and which must arise from the water produced. The chief circumstance, however,

which distinguishes this gas from other inflammable gases, is the great proportion of carbonic acid which it yields when united with oxygen. In the above experiment we find six measures of carbonic acid gas generated; now this quantity would require at least seven measures of pure oxygen, whereas the quantity employed was only $3\frac{1}{2}$ measures; we must therefore infer, that the remaining $3\frac{1}{2}$ measures must have been originally combined with the inflammable gas, constituting what may be called a gaseous oxide. Hence it would appear, that this gas bears the same relation to carbonic acid, that nitrous gas does to the nitrous acid. In future, for the sake of distinction, we shall call it the gaseous oxide of carbone. The gases obtained from the other metallic oxides and charcoal, were perfectly similar in their properties; all of them when mixed with common air and ignited, burned with a lambent blue flame, without any sensible explosion, and much carbonic acid, with a little water, was produced. When mixed with nitrous gas no diminution or sensible charge could be perceived, which proves that their oxygen is in a combined, and not in a disengaged state.

Dr. Priestley has likewise found, that when the carbonate of barytes was mixed with the grey oxide of iron, and exposed to a strong heat in a retort, that not only carbonic acid, but also inflammable gas was obtained. In order to satisfy myself with regard to this fact, I mixed a quantity of the carbonate of barytes and oxide of iron, both having been separately made red hot for some time, and introduced them into a dry coated glass retort. Heat being gradually applied, a quantity of gas, which at first was nothing more than carbonic acid mixed with a little azote, escaped; but when the retort had been red for a few minutes, the gas disengaged consisted of carbonic acid 25 parts, and gaseous oxide of carbone ten parts; the whole amounting to 30 ounce measures. This experiment was repeated, and an iron retort employed, in order that a much greater degree of heat might be given; in this case, not less than 90 ounce measures of a similar mixture of gases were obtained.

Conceiving that in these experiments the gaseous oxide must proceed from the partial decompositions of the carbonic acid by the iron, when raised to a high temperature, I thought I should succeed better by employing iron filings in place of the grey oxide, as these would have a greater affinity for oxygen. Accordingly I took a quantity of common chalk, and exposed it to a low red heat for 10 minutes; it was then mixed with an equal quantity of clean iron filings made as dry as possible; the mixture being introduced into an iron retort, heat was gradually applied. As soon as the retort became red, gas began to be disengaged in great abundance, and continued to come over for a considerable length of time, so that the whole must have amounted to many gallons. Portions of this being examined at different periods, it was found to consist on a medium of one part of carbonic acid gas, and four or five parts of gaseous oxide of carbone.

From this experiment we perceive, that a much greater proportion of gaseous oxide is obtained, and more carbonic acid decomposed, where iron filings are employed in place of the grey calx, which proves that it is the acid which is decomposed by the iron. In another

other instance I substituted lime for the carbonate of lime ; here too the gaseous oxide was obtained, but in much less quantity, and had the lime been perfectly pure, it is probable that very little, or none, would have been procured ; this experiment likewise points out the origin of the gas. In investigating the properties of this inflammable gas, I found that when perfectly free from carbonic acid, it was lighter than common air, in the proportion of 22 to 23.

That when mixed with common air and ignited, it did not explode, but burnt slowly with a lambent blue flame : that when the products of this combustion were collected, by burning a considerable quantity in a large receiver containing common air or oxygen gas, nothing but carbonic acid could be collected, nor was there any perceptible quantity of water condensed against the sides of the vessel. That when 20 parts of this gas were mixed with eight parts of oxygen, and fired over mercury by the electric spark, the whole was reduced to 18 or 19 parts, which were found to be carbonic acid gas, being totally absorbed by lime water. Hence we see, that this gas contains rather more oxygen than that produced from charcoal and finery cinder, as 20 parts of it, with eight of oxygen, produce no less than 18 or 19 parts of carbonic acid gas ; upon the whole, however, they appear to be very similar, more particularly in their specific gravities ; but I remarked, nevertheless, that when a considerable quantity of the gaseous oxide from charcoal, and the different calces was burned in a large receiver, some moisture was deposited on its sides ; there can be little doubt, therefore, that this difference in the results depends on a portion of hydrogen, which enters more or less into the composition of all common charcoal.

The statement at page 7, taken from the foregoing and other experiments, will shew more correctly the difference between these gases, and some of the best known hydrocarbonates ; from these calculations it will likewise appear, that the weight of the gaseous oxide obtained from the decomposition of the carbonic acid, added to that of the pure air necessary for its saturation, is very nearly equal to that of the carbonic acid gas produced ; for 100 cubic inches of this gas weighs 30 grains, and 40 cubic inches of oxygen gas 13.6 grains, making together 43.6 grains. Now 92 cubic inches of carbonic acid gas weighs 43.2 grains ; a greater degree of correctness cannot well be expected from experiments of this nature. But by making a similar calculation with the products obtained from the explosion of the gaseous oxides from charcoal and the metallic calces, it will be found, that the weight of the gaseous oxide, added to that of the pure air, is greater than the weight of the carbonic acid produced ; a circumstance which would seem to prove the formation of water, more especially when compared with the other experiments already related. From these facts, therefore, we would infer, that the purest gaseous oxide is to be obtained from the decomposition of carbonic acid ; for in this case it appears to contain neither water, nor the basis of water.

Dr. Priestley, in his observations on these gases remarks, that in their properties they are very similar to the inflammable air obtained by heat from moistened charcoal ; this, however, upon a more particular investigation, I did not find to be the case. A quantity

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of moistened charcoal being introduced into a coated glass retort, heat was applied, and gradually increased until the retort became red. The first portions of gas collected consisted of nine parts of carbonic acid gas, and 57 hydrocarbonate; what came over about the middle of the process was three parts of carbonic acid, with 55 hydrocarbonate, and the last portions were nearly pure hydrocarbonate, the whole amounting to several gallons.

Having separated the carbonic acid from this gas by repeated agitation with lime water, I found that it was specifically lighter than common air, in the proportion of 11 to 23; in this respect, therefore, it differs essentially from the gaseous oxides, being not more than half their weight; I likewise found, that when burned in a receiver containing pure or common air, a very considerable quantity of water was produced, and condensed against the sides of the vessel; the receiver likewise contained much carbonic acid gas. I next endeavoured to ascertain the proportion of oxygenous gas necessary to saturate it. After several trials I found, when six measures of this gas well washed, were mixed with four of pure oxygen, and exploded over mercury by the electric spark, that the residuary gas amounted to $2\frac{1}{2}$ measures nearly, which was entirely carbonic acid. Here again we find a remarkable difference between this hydrocarbonate and the gaseous oxides, for six measures of oxygenous gas produce only $2\frac{1}{2}$ of carbonic acid, when combined with the former; whereas with the latter, the same quantity would produce no less than 14 measures of the same acid gas. The production of water from the two gases is likewise very different, being little or none from the oxides, but very copious from the hydrocarbonate. Being desirous to ascertain if any of the known hydrocarbonates were similar in their properties to the gaseous oxides, I made comparative experiments with most of them, but there were none which appeared to contain oxygen, at least in any quantity. The purest hydrocarbonates are obtained from camphor, by making its vapour pass through a red hot tube; from ether by the same process; from animal substances, and some vegetables, by destructive distillation; and where it was not expected, from the gas of marshes, for I had always supposed that this gas was similar to that obtained from moistened charcoal by distillation. All these pure hydrocarbonates have exactly the same properties; when well freed from carbonic acid gas, they are lighter than common air in the proportion of 15.5 to 23.5, or 2 to 3 nearly; two parts by measure require no less than $3\frac{1}{2}$ of pure oxygen to saturate them, the products being $2\frac{1}{4}$ parts carbonic acid with some water (see the table) One of their most remarkable properties however, is, that when mixed with $\frac{2}{3}$ their bulk of pure oxygen, or thereabout, and fired in a jar by the electric spark, instead of a diminution, there is a considerable increase of volume, notwithstanding the production of carbonic acid. The following may be considered as the mean result of several experiments made in this way,

Six measures of hydrocarbonate, mixed with $4\frac{1}{2}$ of pure oxygen, were introduced into a strong glass jar over mercury, being fired by the electric spark, the mixture after the explosion, which was violent, was augmented to $12\frac{3}{4}$ measures, or there was an increase of $2\frac{1}{2}$ measures nearly. Lime water being admitted, it soon became turbid, and there was

an absorption of somewhat better than two measures. The remaining gas amounting to $10\frac{1}{2}$ measures, was found by the test of nitrous gas to contain no sensible quantity of disengaged oxygen. Two measures of this residuary gas being fired with one of oxygen, were reduced to one measure, which was found to be carbonic acid gas; hence the whole residuary gas would require about $5\frac{1}{4}$ measures of oxygen to saturate it, and produce five measures of carbonic acid. It would appear, therefore, that notwithstanding the apparent change which the hydrocarbonate underwent in the first explosion, yet the final results are not sensibly varied, for by comparing the two experiments together, it will be found that the proportions of hydrocarbonate, oxygen, and carbonic acid produced, are very nearly as three to one, which they should be where no change is effected. I varied these experiments in different ways, but constantly found the same results.

Alcohol may likewise be converted into a species of hydrocarbonate, by making its vapour pass through a red hot tube; and it is rather singular that this gas should be so different in its properties from that obtained from æther in the same way; for the latter, as has been already observed, is a pure hydrocarbonate.

Having obtained a large quantity of this hydrocarbonate from the decomposition of alcohol, it was well agitated with lime water, but little or no diminution took place, a circumstance which would seem to prove, that pure spirit does not contain oxygen. I next took its specific gravity, and found that it was lighter than common air in the proportion of 12 to 23, whereas that from æther is lighter only in the proportion of 15 to 23. When burned in a receiver containing pure or common air, much carbonic acid, and a considerable proportion of water, were produced. After several trials I found, that four parts of this gas require $4\frac{2}{3}$ of oxygen to saturate them, the products being three parts of carbonic acid, with water. But four parts of hydrocarbonate from æther require seven of oxygen to saturate them, and $4\frac{1}{2}$ measures of carbonic acid with water are produced. Hence the difference between these gases must be manifest; it likewise appears from these experiments, that æther contains less carbone and more hydrogen than alcohol; for it will be found by calculation, that the proportion of carbone to hydrogen in æther is very nearly as 3 to 1, but in alcohol as 4 to 1; indeed this circumstance seems to be confirmed by the deposition of charcoal which takes place in the process for obtaining æther.

From the foregoing experiments, therefore, we perceive, that none of the hydrocarbonates at present known, are similar in their properties to the gaseous oxides of carbone, being much lighter, and yielding a far less proportion of carbonic acid when combined with a given quantity of oxygen.

After acquiring a knowledge of the nature and properties of these gases, we are no longer at a loss to account for their production; we know that the common hydrocarbonates consist of carbone united with, or held in solution by hydrogen, so there can be little doubt that the gaseous oxides are nothing more than carbone united with, or held in solution by oxygen, and then converted into the gaseous state by caloric; that they contain oxygen, is manifest from the very small additional quantity which is found sufficient

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to convert them into pure carbonic acid, from their being procured by a mixture of metallic calces and charcoal in the driest possible state, in which process the metallic calx is found to be revived, having parted with its oxygen to form the gaseous oxide and acid; and from their being obtained by the partial decomposition of this same acid, as in the experiments with chalk and iron filings, &c. From this last circumstance it would also appear, that carbonic acid when raised to a high temperature may be decomposed by any fixed substance which has a considerable affinity for oxygen. Upon the whole, therefore, we consider the gaseous oxide of carbone as holding the same relation to pure hydrocarbonate and carbonic acid; which nitrous gas, or the gaseous oxide of azote, does to pure azote and nitrous acid. We likewise conceive, that the objections to the new system of chemistry advanced on this head, by Dr. Priestley, are sufficiently answered. We find that these gaseous oxides are very different from what the Doctor conceived them to be, and that the presence of water does not appear to be necessary to their production. We have no more reason therefore to suppose that the grey oxide of iron should contain water, than that the other oxides, which give the very same products when mixed with charcoal and distilled, should contain it likewise.

A Table shewing the Analysis, &c. of the different Species of Hydrocarbonates, and the gaseous Oxide of Carbone.

Gases, and the substances from which the gases are obtained, &c. &c.	Weight of 100 cubic inches or measures	Proportion of oxygen necessary to saturate 100 measures of the gas		Products when combined with oxygen				Hence the gases consist of			
				Carbonic acid		Water produc.	Water held in solution by the gas	Oxygen	Carbone	Hydrogen	Water
				in volume	in quant.						
	Grains	Measures	Quantity Grains	Measures	Grains	Grains	Grains				
Pure hydrocarbonate from camphor, &c.	21.	176.	59.8	116.	54.5	18.	8. or 9.	none	11.	2+	8. or 9.
Ditto from æther	20.	170.	58.	108.	50.5	18.	9.	none	9.	3.	8.
Hydrogen from Alcohol	16.	118.	40.	75.	36.	13.	7.	none	7.	1.9	7.
Ditto from wet charcoal	14.5	66.	22.4	40.	19.	9.	9.	none	4 nearly	1.3	9.
gaseous oxides from charcoal and metallic oxides	30.	44.	15.	76.	35.5	about 8.	probably none	about 15.	7.	1+	uncertain
from iron filings and carbonate of lime, or barytes.	30.	40.	13.6	92.	43.2	none	none	21+	8.6	none	none

In the preceding table, the weight of one 100 cubic inches of common air, under the mean pressure of the atmosphere, and at the temperature of 55° feet, is estimated at 31 grains; the quantity of pure carbone in carbonic acid at $\frac{1}{2}$ of the whole nearly, and the proportion of oxygen to hydrogen in water as 85 to 15.

The quantity of oxygen in the gaseous oxides was thus estimated. In the last line, for example, we find that 30 grains of the oxide, produced, when combined with 13.6 grains of

of oxygen, 43.3 grains of carbonic acid. Now this acid consists of 8.6 grains of carbone, and 34.6 of oxygen nearly; but the quantity of oxygen employed was only 13.6 grains, hence the difference between 34.6 and 13.6, or 21 grains, must have been originally contained in the gas. It would therefore appear, that the gaseous oxides in their purest state consist of oxygen and carbone in the proportion of 21 to 9 nearly, combined with caloric.

Woolwich, March 19, 1801.

II.

On the Power of penetrating into Space by Telescopes; with a comparative Determination of the Extent of that Power in natural Vision, and in Telescopes of various Sizes and Constructions; illustrated by select Observations. By WILLIAM HERSCHEL, LL. D. F. R. S.

(Concluded from Vol. IV. page 554.)

JULY 12, 1784. I viewed the same nebula with a large 20-feet Newtonian reflector; power 157.

"A most beautiful extensive cluster of stars, of various magnitudes, very compressed in the middle, and about 8' in diameter, besides the scattered ones, which do more than fill the extent of the field of view*: the large stars are red; the small ones are pale red. RA 18^h 23' 39"; PD 114° 7'."

The penetrating power of the first instrument was 39, that of the latter 61; but, from the observations, it is plain how much superior the effect of the latter was to that of the former, notwithstanding the magnifying power was so much in favour of the instrument with the small penetrating power.

July 30, 1783. With a small 20-feet Newtonian reflector, I viewed the nebula in the hand of Serpentarius, discovered by Mr. Messier, in 1764.

"With a power of 200, I see it consists of stars. They are better visible with 300. With 600, they are too obscure to be distinguished, though the appearance of stars is still preserved. This seems to be one of the most difficult objects to be resolved. With me, there is not a doubt remaining; but another person, in order to form a judgment, ought previously to go through all the several gradations of nebulae which I have resolved into stars."

May 25, 1791. I viewed the same nebula with a 20-feet reflector of my construction, having a penetrating power of $\frac{\sqrt{.64 \times 1881^2}}{2} = 75.08$.

* This field, by the passage of an equatorial star, was 15' 3".

"With a magnifying power of 157, it appears extremely bright, round, and easily resolvable. With 300, I can see the stars. It resembled the cluster of stars taken at 16^h 43' 40" *, which probably would put on the same appearance as this, if it were at a distance half as far again as it is. RA 17^h 26' 19"; PD 93° 10'."

Here we may compare two observations; one taken with the penetrating power of 39, the other with 75; and, although the former instrument had far the advantage in magnifying power, the latter certainly gave a more complete view of the object.

The 20-foot reflector having been changed from the Newtonian form to my present one, I had a very striking instance of the great advantage of the increased penetrating power, in the discovery of the Georgian satellites. The improvement, by laying aside the small mirror, was from 61 to 75: and, whereas the former was not sufficient to reach these faint objects, the latter shewed them perfectly well.

March 14, 1798. I viewed the Georgian planet with a new 25-foot reflector. Its penetrating power is $\frac{\sqrt{.64 \times 240^2}}{2} = 95.85$; and, having just before also viewed it with my 20-foot instrument, I found, that with an equal magnifying power of 300, the 25-foot telescope had considerably the advantage of the former.

Feb. 24, 1786. I viewed the nebula near Flamsteed's 5th Serpentis, which has been mentioned before, with my 20-foot reflector; magnifying power 157.

"The most beautiful extremely compressed cluster of small stars; the greatest part of them gathered together into one brilliant nucleus, evidently consisting of stars, surrounded with many detached gathering stars of the same size and colour. RA 15^h 7' 12"; PD 87° 8'."

May 27, 1791. I viewed the same object with my 40-foot telescope; penetrating power $\frac{\sqrt{.64 \times 480^2}}{2} = 191.69$; magnifying power 370.

"A beautiful cluster of stars. I counted about 200 of them. The middle of it is so compressed that it is impossible to distinguish the stars."

Here it appears, that the superior penetrating power of the 40-foot telescope enabled me even to count the stars of this nebula. It is also to be noticed, that the object did not strike me as uncommonly beautiful; because, with much more than double the penetrating, and also more than double the magnifying power, the stars could not appear so compressed and small as in the 20-foot instrument: this, very naturally, must give it more the resemblance of a coarser cluster of stars, such as I had been in the habit of seeing frequently.

* The object referred to is No. 10. of the *Connaissance des Temps* for 1783, called "*Nebuleuse sans étoiles.*" My description of it is, "A very beautiful, and extremely compressed, cluster of stars: the most compressed part about 3 or 4' in diameter. RA 16^h 46' 2"; PD 93° 46'."

The 40-foot telescope was originally intended to have been of the Newtonian construction; but, in the year 1787, when I was experimentally assured of the vast importance of a power to penetrate into space, I laid aside the work of the small mirror, which was then in hand, and completed the instrument in its present form.

"Oct. 10, 1791. I saw the 4th satellite and the ring of Saturn, in the 40-foot speculum, without an eye glass."

The magnifying power on that occasion could not exceed 60 or 70; but the great penetrating power made full amends for the lowness of the former; notwithstanding the greatest part of it must have been lost for want of a greater opening of the iris, which could not take in the whole pencil of rays, for this could not be less than 7 or 8 tenths of an inch.

Among other instances of the superior effects of penetration into space, I should mention the discovery of an additional 6th satellite of Saturn, on the 28th of August, 1789; and of a 7th, on the 11th of September, in the same year; which were first pointed out by this instrument. It is true that both satellites are within the reach of the 20-foot telescope; but it should be remembered, that when an object is once discovered by a superior power, an inferior one will suffice to see it afterwards. I need not add, that neither the 7, nor 10-foot telescopes will reach them; their powers, 20 and 29, are not sufficient to penetrate to such distant objects, when the brightness of them is not more than that of these satellites. It is also evident, that the failure in these latter instruments, arises not from want of magnifying power; as either of them has much more than sufficient for the purpose.

Nov. 5, 1791. I viewed Saturn with the 20 and 40-foot telescopes.

"20-feet. The 5th satellite of Saturn is very small. The 1st, 2d, 3d, 4th, 5th, and the new 6th satellite, are in their calculated places."

"40-feet. I see the new 6th satellite much better with this instrument than with the 20-feet. The 5th is also much larger here than in the 20-feet; in which it was nearly the same size as a small fixed star, but here it is considerably larger than that star."

Here the superior penetrating power of the 40-foot telescope shewed itself on the 6th satellite of Saturn, which is a very faint object; as it had also a considerable advantage in magnifying power, the disk of the 5th satellite appeared larger than in the 20-feet. But the small star, which may be said to be beyond the reach of magnifying power, could only profit by the superiority of the other power.

Nov. 21, 1791. 40-feet reflector; power 370.

"The black division upon the ring is as dark as the heavens about Saturn, and of the same colour."

"The shadow of the body of Saturn is visible upon the ring, on the following side; its colour is very different from that of the dark division. The 5th satellite is less than the 3d; it is even less than the 2d."

20-feet reflector; power 300.

"The 3d satellite seems to be smaller than it was the last night but one. The 4th satellite seems to be larger than it was the 19th. This telescope shews the satellites not nearly so well as the 40-feet.

Here, the magnifying power being nearly alike, the superiority of the 40-feet telescope must be ascribed to its penetrating power.

The different nature of the two powers above mentioned being thus evidently established, I must now remark, that, in some respects, they even interfere with each other; a few instances of which I shall give.

August 24, 1783. I viewed the nebula north preceding Flamsteed's γ Trianguli, discovered by Mr. Messier, in 1764.

"7-feet reflector; power 57. There is a suspicion that the nebula consists of exceedingly small stars. With this low power it has a nebulous appearance; and it vanishes when I put on the higher magnifying powers of 278 and 460."

Oct. 28, 1794. I viewed the same nebula with a 7-feet reflector.

"It is large, but very faint. With 120, it seems to be composed of stars, and I think I see several of them; but it will bear no magnifying power."

In this experiment, magnifying power was evidently injurious to penetrating power. I do not account for this upon the principle that by magnifying we make an object less bright; for, when opticians have also demonstrated that brightness is diminished by magnifying, it must again be understood as relating only to the *intrinsic* brightness of the magnified picture; its absolute brightness, which is the only one that concerns us at present, must always remain the same *. The real explanation of the fact, I take to be, that while the light collected is employed in magnifying the object, it cannot be exerted in giving penetrating power.

June 18, 1799. I viewed the planet Venus with a 10-feet reflector.

"Its light is so vivid that it does not require, nor will it bear, a penetrating power of 29, neither with a low nor with a high magnifying power."

* This may be proved thus. The mean intrinsic brightness, or rather illumination, of a point of the picture on the retina, will be *all the light that falls on the picture, divided by the number of its points*; or

$C = \frac{I}{N}$. Now, since with a greater magnifying power m , the number of points N increases as the squares of the power, the expression for the intrinsic brightness $\frac{I}{N}$, will decrease in the same ratio; and it

will consequently be in general $N \propto m^2$, and $\frac{I}{N}$ or $C \propto \frac{1}{m^2}$; that is, by compounding $CN \propto \frac{m^2}{m^2} = I = 1$; or absolute brightness a given quantity. M. Bouguer has carefully distinguished intrinsic and absolute brightness, when he speaks of the quantity of light reflected from a wall, at different distances. *Traité d'Optique*, page 39 and 40.

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This is not owing to the least imperfection in the mirror, which is truly parabolical, and shews, with all its aperture open, and a magnifying power of 600, the double star γ Leonis in the greatest perfection.

"It shewed Venus, perfectly well defined, with a penetrating power as low as 14, and "a magnifying power of 400, or 600."

Here, penetrating power was injurious to magnifying power; and that it necessarily must be so, when carried to a high pitch, is evident; for, by enlarging the aperture of the telescope, we increase the evil that attends magnifying, which is, that we cannot magnify the object without magnifying the medium. Now, since the air is very seldom of so homogeneous a disposition as to admit to be magnified highly, it follows that we must meet with impurities and obstructions, in proportion to its quantity. But the contents of the columns of air through which we look at the heavens by telescopes, being of equal lengths, must be as their bases, that is, as the squares of the apertures of the telescopes; and this is in a much higher ratio than that of the increase of the power of penetrating into space. From my long experience in these matters, I am led to apprehend, that the highest power of magnifying may possibly not exceed the reach of a 20 or 25-foot telescope; or may even lie in a less compass than either. However, in beautiful nights, when the outside of our telescopes is dropping with moisture discharged from the atmosphere, there are now and then favourable hours, in which it is hardly possible to put a limit to magnifying power. But such valuable opportunities are extremely scarce; and, with large instruments, it will always be lost labour to observe at other times.

As I have hinted at the natural limits of magnifying power, I shall venture also to extend my surmises to those of penetrating power. There seems to be room for a considerable increase in this branch of the telescope; and, as the penetrating power of my 40-foot reflector already goes to 191,69, there can hardly be any doubt but that it might be carried to 500, and probably not much farther. The natural limit seems to be an equation between the faintest star that can be made visible, by any means, and the united brilliancy of star-light. For, as the light of the heavens, in clear nights, is already very considerable in my large telescope, it must in the end be so increased, by enlarging the penetrating power, as to become a balance to the light of all objects that are so remote as not to exceed in brightness the general light of the heavens. Now, if P be put for penetrating power, we

have $\sqrt{\frac{P^2 a^2}{\kappa}} = A = 10 \text{ feet } 5,2 \text{ inches}$ for an aperture of a reflector, on my construction, that would have such a power of 500.

But, to return to our subject; from what has been said before, we may conclude, that objects are viewed in their greatest perfection, when, in penetrating space, the magnifying power is so low as only to be sufficient to shew the object well; and when, in magnifying objects, by way of examining them minutely, the space-penetrating power is no higher than what will suffice for the purpose; for, in the use of either power, the injudicious overcharge of the other, will prove hurtful to perfect vision.

It is remarkable that, from very different principles, I have formerly determined the length of the visual ray of my 20-feet telescope upon the stars of the milky way, so as to agree nearly with the calculations that have been given *. The extent of what I then figuratively called my sounding line, and what now appears to answer to the power of penetrating into space, was shewn to be not less than 415, 461, and 497 times the distance of Sirius from the sun. We now have calculated that my telescope, in the Newtonian form, at the time when the paper on the Construction of the Heavens was written, possessed a power of penetration, which exceeded that of natural vision 61,18 times; and, as we have also shewn, that stars at 8, 9, or at most 10 times the distance of Sirius, must become invisible to the eye, we may safely conclude, that no single star, above 489,551, or at most 612 times as far as Sirius, can any longer be seen in this telescope. Now, the greatest length of the former visual ray, 497, agrees nearly with the lowest of these present numbers, 489; and the higher ones are all in favour of the former computation; for that ray, though taken from what was perhaps not far from its greatest extent, might possibly have reached to some distance beyond the apparent bounds of the milky way: but, if there had been any considerable difference in these determinations, we should remember that some of the data by which I have now calculated are only assumed. For instance, if the opening of the iris, when we look at a star of the 7th magnitude, should be only one-tenth of an inch and a half, instead of two, then a , in our formula, will be ≈ 1.5 ; which, when resolved, will give a penetrating power of 81,58; and therefore, on this supposition, our telescope would easily have shewn stars 571 times as far from us as Sirius; and only those at 653,734, or 816 times the same distance, would have been beyond its reach. My reason for fixing upon two-tenths, rather than a lower quantity, was, that I might not run a risk of over-rating the powers of my instruments. I have it however in contemplation, to determine this quantity experimentally, and perceive already, that the difficulties which attend this subject may be overcome.

It now only remains to shew, how far the penetrating power, 192, of my large reflector, will really reach into space. Then, since this number has been calculated to be in proportion to the standard of natural vision, it follows, that if we admit a star of the 7th magnitude to be visible to the unassisted eye, this telescope will shew stars of the one thousand three hundred and forty-second magnitude.

But, as we did not stop at the single stars above mentioned, when the penetration of the natural eye was to be ascertained, so we must now also call the united lustre of sidereal systems to our aid in stretching forwards into space. Suppose therefore, a cluster of 5000 stars to be at one of those immense distances to which only a 40-feet reflector can reach, and our formula will give us the means of calculating what that may be. For, putting S

* Phil. Trans. Vol. LXXV. page 247, 248.

for the number of stars in the cluster, and D for its distance, we have $\frac{\sqrt{N} \times A^2 S}{a} = D^*$; which, on computation, comes out to be above $11\frac{3}{4}$ millions of millions of millions of miles! a number which exceeds the distance of the nearest fixed star, at least three hundred thousand times.

From the above considerations it follows, that the range for observing, with a telescope such as my 40-feet reflector, is indeed very extensive. We have the inside of a sphere to examine, the radius of which is the immense distance just now assigned to be within the reach of the penetration of our instruments, and of which all the celestial objects visible to the eye, put together, form as it were but the kernel, while all the immensity of its thick shell is reserved for the telescope.

It follows, in the next place, that much time must be required for going through so extensive a range. The method of examining the heavens, by sweeping over space, instead of looking merely at places that are known to contain objects, is the only one that can be useful for discoveries.

In order therefore to calculate how long a time it must take to sweep the heavens, as far as they are within the reach of my 40-feet telescope, charged with a magnifying power of 1000, I have had recourse to my journals, to find how many favourable hours we may annually hope for in this climate. It is to be noticed, that the nights must be very clear; the moon absent; no twilight; no haziness; no violent wind; and no sudden change of temperature; then also, short intervals for filling up broken sweeps will occasion delays; and, under all these circumstances, it appears that a year which will afford 90, or at most 100 hours, is to be called very productive.

In the equator, with my 20-feet telescope, I have swept over zones of two degrees, with a power of 157: but, an allowance of 10 minutes in polar distance must be made, for lapping the sweeps over one another where they join.

As the breadth of the zones may be increased towards the poles, the northern hemisphere may be swept in about 40 zones; to these we must add 19 southern zones; then, 59 zones, which, on account of the sweeps lapping over one another about $\frac{1}{5}$ of time in right ascension, we must reckon of 25 hours each, will give 1475 hours. And, allowing 100 hours per year, we find that, with the 20-feet telescope, the heavens may be swept in about 14 years and $\frac{3}{4}$.

Now, the time of sweeping with different magnifying powers will be as the squares of the powers; and, putting p and t for the power and time in the 20-feet telescope, and $P = 1000$ for the power in the 40, we shall have $p^2 : t :: P^2 : \frac{t P^2}{p^2} = 59840$. Then, ma-

* $D = 11765475948678678679$ miles.

king the same allowance of 100 hours per year, it appears that it will require not less than 598 years, to look with the 40-feet reflector, charged with the abovementioned power, only one single moment into each part of space; and, even then, so much of the southern hemisphere will remain unexplored, as will take up 213 years more to examine.

Slough, near Windsor, June 20, 1799.

III.

*Enquiries relative to the Laws of Affinity. By CITIZEN BERTHOLLET, Member of the National Institute, &c. Abridged by CIT. BOUILLON LAGRANGE.**

THE publication of the work of Cit. Berthollet on the affinities, has long been expected by chemists. Their anxiety was doubtless very natural; for an object so important could only be completed by that celebrated individual, whose discoveries have been so highly advantageous to society.

The work here announced cannot be abridged without difficulty; for by presenting the reader with a mere analysis of each article, much information would be lost. Hence we shall frequently suffer the author to speak for himself, as his work is a collection of facts, upon which the theory is established.

Cit. Berthollet has divided his memoir into fifteen articles:

In the first of these, the author makes a panegyric upon Bergmann, who devoted his attention to this subject with the greatest success. Cit. Berthollet has principally attended to the work of the Swedish chemist, when treating on this subject. He then states the object of his memoir: "It is my intention" says he "to prove that the elective affinities do not act like absolute forces, by which one substance might be displaced by another in a combination; but that in all the compositions and decompositions which are owing to elective affinity, a division is made of the object of the combination between the substances whose actions are opposed; and that the proportions of this division are determined not only by the energies of affinity of these substances, but also by the quantity with which they act; so that the quantity may compensate for a want of the affinity, and produce a similar degree of saturation.

If, continues the author, I establish that the quantity of a substance can supply the want of force of its affinity, it will follow that its action will be proportionate to the quantity necessary to produce a determinate degree of saturation. I denominate *mass*, that quantity which is the *measure* of the capacity of the saturation of different substances.

When therefore I compare the affinities of substances, I shall direct my attention to the ponderable quantity, which in this comparison ought to be equal; but, when I compare

* Annales de Chimie, XXXVI. 302.

their action, which is composed of both their affinity and their proportion, I must then consider the mass.

The author next observes, that in the following discussions, he shall principally make use of acids and alkalis, among which he comprises those earths which act as such; because they exercise great powers, which may dispel the influence of slight causes; and because they often produce comparable degrees of saturation, and afford results which may be easily observed. But the consequences drawn from their properties by Cit. Berthollet, are applicable to all other combinations. It is proved by many examples, that the principle established by the author extends to the whole chemical action of bodies.

After having proved by direct experiments, that the chemical action of substances, of which the forces are opposed, does not depend upon their affinity alone, but likewise on their quantity, the author states that he shall make choice of observations on different kinds of combinations which will confirm this principle, and prove its extent. I shall examine, he continues, the circumstances which modify it, or those affections of bodies which favour or diminish their chemical action, and which cause the proportions of the combinations which they are capable of forming to vary: I shall apply these considerations to the complex affinities, and to those of compound bodies; and lastly, I shall endeavour to determine the basis on which the general and particular theories of chemical phenomena ought to be established.

ARTICLE II.

Experiments which prove that in the Elective Affinities, the opposing Substances divide that Body which is the Subject of the Combination.

To demonstrate the truth of this proposition, Cit. Berthollet describes several experiments, from which we shall quote the following: I kept, says he, in a state of ebullition in a small quantity of water, an equal weight of pot-ash, purified by alcohol, and of sulphate of barytes. The operation was made in a retort, and consequently without the contact of air, and continued till the mixture was evaporated to dryness: the residuum was treated with alcohol, which dissolved the pot-ash, and afterwards with water. The latter effected a solution which also exhibited alkaline properties. This alkali was saturated with acetous acid, after which there was formed, by evaporation, a considerable quantity of small crystals, which possessed all the characters of sulphate of pot-ash; so that the sulphate of barytes was partly decomposed by the pot-ash, and the sulphuric acid was divided between the two bases.

The other experiments were made,

1. With sulphate of pot-ash and lime: 2. With oxalate of lime and pot-ash: 3. With oxalate of lime and nitric acid: 4. With phosphate of lime and pot-ash: 5. With pot-ash and carbonate of lime: 6. With soda and sulphate of pot-ash.

In the whole of these experiments it is seen that those bases, which are admitted as forming, with the acids, the strongest and, most permanent combinations, were partly

extricated by a basis to which a weaker affinity is attributed; so that the acid was divided between the two bases. It is likewise observed that the acids are partly disengaged from their base by others, the affinity of which is considered as inferior, so that the base is divided between two acids.

The author premises, that if a small quantity only of the decomposing substance be employed, the effect will not be perceptible, and he concludes, that when any substance acts upon a combination, that which is the subject of the combination is divided between the two other substances, not only according to the respective energies of their affinity, but likewise according to their quantity.

A R T I C L E III.

Observations which confirm the Principle of the Chemical Action in Proportion to the Mass.

The author makes observations on different kinds of combination exposed to an elective affinity, and examines whether the principle of chemical action, in proportion to the mass, be not precisely applicable to their explanation.

If, as he observes, we treat the carbonate of pot-ash with lime, we cannot deprive the pot-ash of all its carbonic acid, even by making successive operations with fresh lime; and if we evaporate the fluid, the residuum is still capable of effervescence, on being saturated by the acids, because the pot-ash which remained present with the lime opposed its action; and, the greater the quantity of carbonic acid which has been before absorbed by the lime, the more powerful is the pot-ash to maintain its combination with this acid.

When the equilibrium is established between the action of the lime and the resistance of the pot-ash, if the liquid be filtered and evaporated, we may take away, by a weak affinity, that portion of alkali which is superabundant to the constitution of carbonate of pot-ash, that is to say, the whole portion which is not defended by a considerable mass of carbonic acid. Alcohol possesses this property; the separation is effected by this fluid. The carbonate of pot-ash remains in solution in a small quantity of water, while the alcohol of pot-ash floats above. The separated carbonate may again be treated with lime; and by this second operation it may be reduced to an inconsiderable quantity.

Cit. Berthollet also quotes several other experiments relative to the chemical action in proportion to the mass.

If a carbonate which contains an excess of pot-ash be treated with alcohol, it can only be deprived of a part of its excess by such treatment.

The other neutral salts likewise possess the property of retaining a part of that pot-ash which may be contained in them superabundant to their neutralization.

It is also known that phosphate of lime cannot be entirely decomposed by the sulphuric acid; though it has been asserted that the sulphuric acid has a greater affinity to lime than the phosphoric acid.

The

The same happens when the sulphate of alumine is decomposed by ammonia: the precipitate always contains a portion of sulphuric acid.

If the magnesia of the sulphate of alumine (qu. magnesia?) be precipitated by pot-ash, the magnesia will also contain a portion of sulphuric acid; for when the magnesia is submitted to strong heat, it is afterwards found to possess rather a strong taste of sulphate.

All these experiments prove, that in chemical analyses we are liable to error, by taking that for the real weight, of alumine or magnesia, which may have been contained in a compound substance, and is merely the precipitate formed by elective affinity.

From the preceding observations, and many others which cannot here be given, it appears that in operations of elective affinity the subject of the combination is divided between the two substances which act upon it, in consequence of the forces they are capable of opposing to each other.

A circumstance worthy of attention, and which clearly proves that the chemical action depends as much on the quantity as on the affinity of substances, is, that to produce opposite effects, it is only necessary to make the requisite alteration in the quantities.

A R T I C L E, IV.

On those Modifications of Chemical Action, which arise from the Insolubility of the Substances.

The author successively examines those affections of bodies which may disguise or modify the applications of the principle established in the preceding articles. He proves by a number of experiments; 1. The manner in which an insoluble substance acts when opposed to a combination. 2. If a substance possesses some degree of solubility, its action is composed of that of the dissolved part, and of that which remains solid. 3. If an insoluble combination be attacked by a liquid substance, the inconveniencies of its insolubility speedily disappear, in the case when it is only necessary that the insoluble substance should lose a part of its constituent principles, to become liquid.

The author next proceeds to examine the difference of specific gravities of the insoluble substance, and the liquid. According to him, this has an influence on their respective actions, even when agitation and heat are employed; because it continually tends to separate the insoluble substance, and to withdraw from the action opposed to it. Hence there is a difference on this account between the sulphates of barytes and of alumine.

Lastly, as the insolubility prevents those proportions of division from taking place, which might else have been established by the opposing forces, so likewise it is found to retard the operation of those which can take place; so that it may easily produce deception by the appearances which it affords at the commencement of an operation. For example, when we mix some concentrated sulphuric acid with a solution of sulphate of pot-ash, or any other salt which requires a large proportion of water to dissolve it, the acid soon combines with the water; and the salt which loses its liquidity is precipitated; but by continuing the experiment, and multiplying the contacts, the salt becomes dissolved, and enters into combination with the liquid.

ARTICLE V.

On Cohesion and Crystallization.

The cohesion of the particles of a body is owing to the mutual affinity of those particles: it is a force which must be overcome by the action of the substance which has a tendency to combine with those parts, or to decompose their combination. It is known that clay, the particles of which have formed a strong cohesion by desiccation, is no longer attacked by an acid which would be capable of dissolving it when in another state.

The mutual affinity of the saline particles is the cause of crystallization; and this modifies the chemical action in a manner which deserves attention. The author makes some remarks on this subject, as well as on the force which produces crystallization in a saline solution. It establishes a limit to which the degree of saturation of the salt in water can arrive; so that if the water does not dissolve a greater quantity of it, this is not in consequence of its affinity being satisfied, but because it is no longer sufficiently powerful to overcome the resistance of the crystallization. From these and numerous other considerations, Cit. Berthollet concludes that the force of cohesion, which has hitherto been considered merely as an obstacle to solution, determines the quantities of substances which may be brought into action in a liquid; and thus modifies the conditions of chemical action. It is this also which causes those separations that take place, either by crystallization or precipitation, and determines the proportions of the combinations which are formed by separation from the liquid, when the solubility or insolubility depends upon these proportions.

ARTICLE VI.

On the Elasticity of Substances which exert a Chemical Action.

When any substance escapes in a gaseous state, in proportion as it is disengaged from an intimate combination, the whole of that part which assumes the elastic state, ceases to afford any resistance; so that the substance no longer acts in the ratio of its mass. The substance presented may in this case render the decomposition complete; and it will be sufficient to use simply that quantity which would have been requisite for immediately forming the combination in which it is to enter, or at least a small excess only will be necessary.

This happens, as Cit. Berthollet observes, with the carbonic acid, when it forms a carbonate, and another acid is presented to it. This last acid, which acts in proportion to its mass, may, even with an affinity inferior to that of the carbonic acid, successively expel it from the combination, even till none shall remain, provided it be employed at first in a quantity rather greater than would have been necessary for immediately forming its combination with the basis:

From all these facts presented by the author, it follows that elasticity produces effects analogous to those of the force of cohesion, by modifying, in an opposite direction, the effects of the affinity proper to each substance.

ARTICLE VII.

Of the Action of Caloric.

In this chapter Cit. Berthollet examines the action of caloric upon bodies, and the phenomena it produces in the various combinations or decompositions. Articles VIII. and IX. contain a great number of experiments on efflorescence and on solvents.

What Cit. Berthollet understands by efflorescence, is the property of rising above the mass, and of separating from it by chemical action.

In order that soda may rise in efflorescence, he observes, it must necessarily have combined with carbonic acid, which it can attract from the air of the atmosphere; but the action of the carbonic acid, which is very small in quantity, and in an elastic state, cannot sensibly add to the force which has produced the separation of the soda, from the combination in which it is found; it only separates the eliminated part, and hinders it from continuing to act upon the combination.

With respect to solvents, the author is of opinion that their use is to overcome the resistance which arises from the cohesion of the parts intended to be brought into action, or from their elasticity, and to increase their mutual contact.

Solvents act upon substances, which they dissolve by their affinity and their quantity, like all substances which have a tendency to combination; and to these is applicable all the doctrine of the combination of bodies. The author takes water for his example, which is most frequently employed as a solvent.

Cit. Berthollet then examines the circumstances in which this action may change or sensibly modify the results; and this leads him to offer a series of experiments; on which he establishes a luminous theory.

This article is concluded by some remarks upon caloric. The author conceives it to act upon bodies which are not equally dilatable in a manner similar to solvents, by attacking the force of cohesion, and thus enabling the particles to exercise their mutual action. Its action concurs with those of the solvents, in opposing the force of cohesion: whence it happens, that the solution of a salt by water varies according to the degrees of temperature.

When caloric acts upon substances not equally dilatable, it produces separations and new combinations, independent of the affinity peculiar to substances; in the same manner as the solvents when they act upon substances not equally soluble.

(To be continued.)

Supplementary

IV.

Supplementary Letter on the Syphon, to the Communication at page 547 of the last Volume.

By Mr. WILLIAM CLOSE.

TO MR. NICHOLSON,

S I R,

VALVES covered with leather may be adapted to the ends of the small pipes, and they may be opened by drawing a wire after the external valve has closed its tube. For this purpose a long small pipe must be fixed into the bottom of A W, (Pl. XXIV. Fig. 1.) must rise to near the top of that vessel, and its lower end be immersed in a vessel of water below the lowest part of the syphon. The wire must pass through this pipe, and be fixed to one lever within the vessel for raising the valves, and to another below the bottom of the pipe, connected with the moveable vessels above. When the second and fourth vessels ascend, the valves will open, unless they are prevented by the weight of water in the syphon, and water will rise in the small wire pipe, but cannot enter the vessel A W. The ascent, however, will extend through a space nearly equal to the length of the descending column in the syphon.

A very easy and extensive method of rendering active the pressure of the atmosphere is here presented to our notice. The vessel A W may be filled with water through a small vertical pipe, without receiving any from the syphon, to which it need only be connected by an air pipe.

By the same principle water may be raised to a considerable height above the syphon. To return, however, to our former subject, the water in the pipe will completely prevent the admission of air into the receptacle of raised water: it serves the purpose of packing or leathering.

The water in the vessel A W, will never reach the higher end of this pipe, because it cannot rise above the air pipe connected with the syphon.

If such a long pipe should be inconvenient, a much shorter one may be used, if its lower end be immersed in mercury instead of water; it may then also be considerably wider than the other, and an iron rod of a rather less diameter may be placed within it to push up the valves. If the valve of the water tube be made light, it will open and shut without any assistance.

The vessel A W. should not be large; the top part of it should be fixed on with screws, to allow the valves to be easily taken out and repaired. Two cocks in the small pipes opposite A and W will then be useful to prevent the depression of the columns in the branches of the syphon.

It will be always adviseable to make the length of the descending branch of the syphon exceed that of the ascending one, as much as circumstances will admit, and to let the lowest part of it be made of a conical divergent form: the velocity of the current will be thus encreased, the vessel will be sooner filled, and the depression of the columns will be less liable to happen from very slight imperfections in the valves.

I am, SIR,

Your humble servant,

Dalton, Feb. 21, 1801.

WILLIAM CLOSE.

CORRECTION.

There was a slight omission made in one paragraph of the letter before this, which may easily be corrected. Please to look for the following words at the beginning, page 547.

If the connexion between the vessel and syphon was made by two tubes *inserted into the shorter branch one immediately above the other*, the end of the *lower* turned against the current, and the end of the *higher* turned from it, the stream, &c. Insert the words with the line under them.

V.

On the Doctrine of Heat; particularly with regard to the States of dense and elastic Fluidity in Bodies. In a Letter from Mr. JOSEPH ASTLEY.

TO MR. NICHOLSON.

S I R,

THAT the solid, liquid, and aeriform modifications of bodies depend on nothing more than the different proportions of caloric with which they are directly combined, is an opinion so attractive, from its apparent simplicity, and so closely associated with our most elementary notions respecting the physical laws of nature, that a degree of prejudice may perhaps be excited in many by the mere suggestion of a doubt upon the subject. Those intimate with chemical theories have learned, however, that the apparent simplicity of a doctrine is as frequently the result of an imperfect as of intire comprehension of the subject which it regards, and that our solicitude to establish the conformity of particular phenomena too often leads us to admit inconsistencies into our general principles of physical economy. The new lights which have been lately thrown on several of the phenomena in which evaporation is concerned, particularly on those of meteorology, have been sufficient to

create suspicion in the minds of many who have considered them attentively, that the doctrine which affirms this process to consist merely in a direct combination of caloric is not to be admitted without limitations. They have regarded these limitations, however, as only partial exceptions to a general law: and my present purpose is to show that, even in its most general acceptance, the doctrine is open to very weighty objections.

I shall endeavour to explain myself in examining what grounds we have for the supposed analogy between vaporization and liquefaction; what indications we have as to the real action of caloric in vaporization; and whether the probability both of analogy and of facts should not incline us rather to refer the vaporous modification of bodies to the class of chemical decompositions.

The most important and characteristic circumstance attending the phenomenon of liquefaction is the uniformity of temperature at which it takes place. And it is here that a most obvious failure in the supposed analogy of vaporization presents itself. It is not sufficient for this analogy that in similar circumstances water, like ice, cannot preserve its form beyond a certain temperature. It would require also that water, like ice, should not be capable of changing its form below that temperature. So little do we find this the case, that many circumstances connected with evaporation give us reason to conclude that, when not impeded by mechanical resistance, there is scarcely any temperature at which it may not take place; and that, without any variation but as to the rapidity with which it is performed. I shall enter into no examination here of the attempt to rescue the analogy in question from this objection by referring what is called the spontaneous evaporation of liquids to the solvent action of air, because in as much as respects the principle of the objection, the phenomenon is equally evident whether air be present or not.

But to come more directly to the point, let us attend to the circumstance by which supposed analogy between liquefaction and evaporation is deemed peculiarly expressed. This is the appearance in each case of a quantity of combined caloric. It will be seen in the sequel that I am far from being disposed to deny this combination in the phenomena of evaporation. All that I have to object to is the idea of its indicating a correspondence of principle between these phenomena and those of liquefaction. The appearance from which the combination of caloric in the former cases is inferred, are those of its becoming latent in the production, and again sensible in the condensation of vapour. It must be observed, however, in the first place, that the caloric concerned in these two phenomena is by no means the same. Mr. Watt found that vapour produced in vacuo at a temperature 70° , gave out, on its condensation, a quantity of caloric equal to 1200° or 1300° . And, as it must be needless to show that such a quantity could not have existed in the vapour on its original production, it is evident that this at least cannot be regarded as having any essential relation to its constitution.

Let us see how far the caloric which is actually present, and becomes latent in the formation of vapour, can be understood to denote a conformity between this modification and that of liquefaction. That this is not implied by the mere circumstance itself, is evident

evident from the variety of cases in which we see it to take place during chemical changes of the most different kinds, and even in some modifications which must be accounted purely mechanical. Before, therefore, the latent combination of caloric can serve as any index of physical analogy between two phenomena, there must be shown to exist a correspondence in the other changes with which it is respectively associated. So little do we find of this correspondence here, however, that the changes most intimately and essentially connected with it are in the one case an expansion, and in the other a diminution of bulk.

Nor do we find the comparison at all better supported, if, attributing the latent absorption of caloric to a previous change of capacity, we attempt to substitute this as an indication of the analogy in question*. This change of capacity itself requires, no less than the absorption of caloric, the supposition of a previous modification, and if, with the generality of authors, we refer this modification to change of form, the same difficulty occurs as before, viz. that of conceiving how a similar change of form can be attended with augmentation of volume in one case, and diminution of it in the other. Besides that to ascribe the change of capacity in vaporization and liquefaction to change of form, instead of bringing us to the cause of the phenomena, is only leading us back to the phenomena themselves, and thus returning to the same point from which we set out. The only way by which it has been attempted to escape from this circle has been by resting the immediate occasion of the phenomena on the change of temperature: and how very little this is to be trusted to as an index of analogy between vaporization and liquefaction, has already been seen.

But we may easily satisfy ourselves of the fallacy of the supposed analogy between these two modifications of any body, if we attempt to infer from it the correspondence of the changes which such a body should exhibit with respect to both in consequence of the same changes in the constitution. Thus, if the analogy were to be depended on, the same change of constitution by which the freezing point of water becomes lowered, should lower also its boiling point. I need not point out how grossly such inferences would mislead us. And if the solvent action of the air may serve to screen this analogy from the absurdity of some of these implications, it has at the same time introduced other inconsistencies which are equally gross. According to this, the influence of air and of caloric in producing evaporation being similar, we should expect that when the action of the former is added to that of the latter, the evaporation should be more speedy than when the latter exerts itself alone. In other words, that the same quantity of evaporation should be

* How little information we derive, however, from being told that the absorption of caloric is a consequence of increase of capacity, is easily seen by attending to the meaning of the term. An increase of capacity is a power of combining with a greater proportion of caloric at the same temperature. The whole secret of the explanation therefore consists in this: that solid bodies, in becoming liquid, combine with a greater proportion of caloric at the same temperature, because they thereby *acquire the power* of combining with a greater proportion of caloric at the same temperature.

produced in the presence of air by a less quantity of caloric than is required in its absence. The contradiction of this, by experiment, is explained by telling us the air, being after a short time saturated with water, acts merely by its mechanical pressure, and that to overcome this pressure the vapour requires the combination of a greater proportion of caloric. Yet, after all, we find that vapour in the formation of which this excessive quantity of caloric has been employed, manifests considerably less caloric in its condensation than vapour produced in vacuo with a heat of 70° .

In what, then, have we reason to believe the action of caloric in evaporation to consist? From what has been already said, and particularly from the experiment just noticed, we have, I think, reason to conclude that no known phenomena authorize us to refer it to any direct specific combination. Were this supposed, it would surely be expected that we should assign some determinate proportion to such combination. If this cannot be done, if we can in no respect so proportion the caloric applied that it shall uniformly produce the same modification, however we may think ourselves able to account for this difference of effect, our explanation must inevitably imply that there is some condition more essential to evaporation than the union of caloric. Thus, as in the explanation of the impediment which evaporation sustains from the pressure of the air, it cannot be pretended that this acts by preventing in the first instance the direct combination of caloric, but by preventing this combination from producing the effects which would otherwise accompany it, we are fully authorized to conclude that the direct combination of caloric is not, as the ordinary theory of this phenomenon, and as all analogy drawn from the phenomena of liquefaction would imply, the determinate specific cause of evaporation.

It becomes interesting, therefore, to investigate that more essential condition of this phenomenon, which the common character of all the circumstances which modify it indicates to be *expansion*: and, since we find that it is by the degree in which this condition is induced, and not by the quantity of caloric actually present that the agency of the latter in evaporation is to be measured, and since we find that expansion has a constant tendency, even independently of caloric, to cause evaporation, we must expect to derive, from tracing the modifications which this condition implies, our most comprehensive ideas of the principles of these phenomena.

Viewing the expansion of a body in its most simple light, merely as indicating a diminution in the attractive tendencies of its particles, there are two ways in which our knowledge of these tendencies suggests to us that it may be effected: one, by a change in the structure or configuration of the particles themselves; the other, by a change in the

* The influence of the air has indeed been an equal subject of perplexity with those who have attempted to associate it with the agency of caloric in evaporation, and with those who have excluded it from this association. If we would believe the most celebrated advocate of the latter opinion, it is alike to the mechanical influence of the air (for he denies it to have any influence but mechanical) that we are to attribute the liquefaction of vapour at one degree of pressure, and its power of resisting liquefaction when subjected to a greater. See De Luc's *Theheocalogie*, Vol. I. p. 20, &c.

mutual relation of the principles of which these particles are constituted. And this consideration inculcates an important distinction between two processes, which have been hitherto, if not altogether confounded, separated at least by a distinction worse than none: I mean sublimation and vaporization. The slightest attention will show the absurdity (supposing it the same in both) of calling by different names the action of caloric upon bodies as they are in a solid or in a liquid state. Nor would so futile a distinction probably ever have occurred, had not a real specific difference of action in the two cases been observed to be generally denoted. In the former, nothing but a minute division of the parts (which still retained in other respects all their former characters) was observable: whilst, in the latter, there was not merely a comminution of parts, but a total diversity of appearance. If we suppose the action of caloric to be in the former process, which comprehends all the simplest bodies, that of merely *changing the structure and configuration of the particles*, whilst in the latter it exerts itself in *changing the mutual relation of the constituent principles* themselves, shall we not have at once an explanation of the physical nature of this distinction, and a satisfactory clue to some of the most important phenomena of nature?

Before we advert to the evidence of facts, let us see how far analogy would carry us in making such a change in the constituent principles of compound bodies as a probable account of the action of caloric in their vaporization. As the ordinary processes of distillation so frequently exhibit to us the separation of the parts of chemical compounds from each other, according to their respective degrees of volatility, and as we can understand nothing by the different volatility of substances but their different affinities for caloric, it is obvious that as far as the first general dictate of analogy is to be trusted, we are decidedly authorized to expect that all bodies, whose constituent parts possess different affinities for caloric, should be liable to the same decomposition. And such an inference can be objected to only on the ground of some palpable inconsistency with the evidence of facts themselves. The construction hitherto put upon this evidence has been, that bodies which, in common language, preserve their constitution entire in evaporation, combine with caloric not according to the discrete affinities of their several parts, but according to their aggregate specific affinity for it. How far this assertion is warranted by the direct testimony of fact will be examined presently. In the mean time it may be remarked that not only the general analogy just noticed, but all the individual analogies also, which are any way concerned, unite in contradicting it. According to this assertion, the constituent affinity of the parts of water (for instance) for each other should be so great as entirely to supersede the difference of their respective affinities for caloric. But this is not at all consistent with the circumstances which occur in its synthetical formation; where foreign aid is always necessary to establish the superiority of the former; and where the caloric conveyed out during the union of the parts gives us strong reason to presume that its re-combination must indicate their dissolution. Besides, the mere circumstance of expansion accompanying the combination of caloric sufficiently indicates its tendency to dissolve in

greater or less degree not only the cohesive but the constituent affinities of compound bodies*. In fact, if we consider attentively, we shall find that we can form no accurate conception of a change produced in the former which shall not affect proportionally the latter. As the cohesive affinity of a compound body owes its existence to the constituent affinities of its parts, the idea of a diminution in the former includes in itself the idea of a correspondent diminution in the latter.

Thus, then, we find that every suggestion of analogy would lead us, *à priori*, to expect that a partial or complete disunion of parts should always attend the combination of compound bodies with caloric; and, consequently, that their evaporation is to be regarded as a true decomposition. In submitting it to the direct testimony of facts, however, the phenomena of evaporation will be deemed in two respects irreconcilable with the theory which these analogies would suggest. The different parts of such bodies, it will be said, do not escape in that succession which their different degrees of volatility would thus indicate: nor does their appearance after the process accord with the idea of their decomposition. With respect to the latter, however, to infer that there is no decomposition, because the substance collected after evaporation presents the same appearances which it had before, is not an admissible deduction: since the vapour cannot be so collected without having its caloric attached; and as the theory supposes the combination of caloric competent to the disunion, it implies equally that the separation of caloric will be effective of the re-union of the component principles.

But there are also positive facts to be opposed to the objection. Numerous instances of evaporation occur in which we do see evident marks of decomposition, and of a decomposition which we can ascribe to nothing but the combination of caloric. Dr. Priestley's experiments on the generation of air from water, viewed independently of the theory which he connected with them, afford strong illustrations of it. The ready disunion of the parts of vapour by many substances incapable of decomposing it in the state of water; as well as the indications which Count Rumford has noticed of the decomposition of water in operations of cooking, must be admitted as at least presumptive proofs. And the combustive qualities of the vapour of the eolipile, and several phenomena of a similar kind, present evidences of decomposition sufficiently satisfactory†. Nor are the evidences of a similar

* I did not know till after I had written the above, that Fourcroy, in speaking of the capacity of hydrogen for caloric, had expressed himself in the following manner: "It even appears that this base, though combined with oxygen in water, still possesses the property of absorbing a great deal of heat. And that it is this property which renders vapour specifically lighter than air." Discourse on Modern Chemistry.

† To these may be added the very important inferences to be drawn from meteorological phenomena, respecting the decomposition of water in what is called spontaneous evaporation. I must acknowledge myself indebted for much light on these phenomena to a very ingenious paper on the subject, in the Essays by a Society at Exeter, and I am disposed to differ from the author only on those points in which he seems to differ from himself. The true tenor of his arguments is certainly to prove that water exists in a decomposed state

similar decomposition in the evaporation of other bodies less conspicuous. The vapour of ether, according to Lavoisier's experiments (Mem. de l'Ac. des Sciences, 1780) becomes, by the mere admission of common air, a permanently elastic fluid, and in this state deflagrates on the application of flame, like a mixture of oxygen and hydrogen gases. The very interesting experiments of the Dutch chemists on the decomposition of alcohol and ether, in the passage of their vapours through tubes of glass or of porcelain, may be considered as affording evidence to the same general purport.

In answer to the other part of the objection, that if substances be considered as decomposed in evaporation; their parts should escape successively, according to their different affinities for caloric, and not altogether, as is the case, it might be sufficient to observe, that the escape of one from the remainder in an uncombined state, all our observations of other phenomena teach us that this state must necessarily be that of an elastic gas, and therefore the aeriform conversion of both parts must take place together. But we may go further, and affirm that it is to the phenomena, as thus explained, that we must have recourse, in order to understand one of the most important and distinguishing circumstances, by which the process of evaporation is marked. I mean the generation of cold. And if this can be shewn, the theory which I have been attempting to explain will have something more than analogy for its support.

state in the atmosphere. Yet he seems to have formed his idea of it as "a permanent gas, unchangeable, like other compounds, except by an elective attraction superior to that which unites its ingredients," page 368. And in this light we cannot wonder at his expressing his inability to explain "what occasions the formation of water in the air, to admit of its falling in rain at any time." Without professing to enter at all into the *rationale* of this phenomenon, I may observe that the principal difficulties with which it seems embarrassed in his theory will be removed, if we consider water to enter into the atmosphere, not as a *permanent compound gas*, but as decomposed into the two original permanent gases belonging to its constitution. These, having different specific gravities in the atmosphere, must be supposed to occupy different regions in the atmosphere, which, however, will vary, and occasionally intermingle with each other by the operation of different circumstances. In such cases the affinity between their respective bases, when aided by any occurrence disposing them to part with the caloric which separates them, may be easily supposed to occasion a re-union.

According to this author's idea of the air proceeding from the evaporation of water being a permanent gas, to what can he assign its specific distinction from water itself? Will he say in its combination with caloric? But he has shewn that it is both producible without the assistance and irresolvable by the subtraction of caloric. Besides, we have positive evidence that this gas, in its original state, is not permanent. The vapour immediately rising from boiling water is this gas in its original state, transparent and elastic. But it will not be said that this is incondensable by cold. It becomes permanent only by the local separation of its parts. And if we admit this separation, I think we not only see the arguments which the author has brought together to illustrate the modifications of the atmosphere by evaporation in a stronger light, but we see the analogy of several other modifications with the same general source. We see in particular why a greater quantity of oxygen is observed in the atmosphere during dry serene weather than during the prevalence of clouds, fogs, or rains; and, connected with this, why the air of lower regions has been always found charged with more oxygen than that of more elevated ones.

It requires little attention to discern that the fancied analogy between the cold which is produced in evaporation and in liquefaction, has in reality nothing for its support but the generality of occurrence in each phenomenon. The cold which takes place in liquefaction is a mere consequence of the subject of the liquefaction being surrounded by bodies of a higher temperature than its own, whose caloric therefore is in some part detached, on the general principle of its tendency to equilibrium *. In evaporation, on the contrary, the circumstances are nearly the reverse. The subject of this process has in general a higher temperature than that of the substances around it. The tendency to equilibrium between these will not account therefore, as in the case of liquefaction, for the cold which takes place. We must look forward for the occasion of it to the product of the operation.

We shall be assisted in our examination of this by premising, that we have no instance in the whole physical history of nature of a substance producing cold in consequence of any difference between its capacity and that of the surrounding bodies, unless it have at the same time a temperature actually inferior to their's. I have already shown that the cold produced in the liquefaction of ice is to be referred to this: and we may easily satisfy ourselves that in no frigorific phenomena whatever have we reason to ascribe it to any thing else. Thus in mixtures of salts and snow, it is not the change of capacity which is the proximate cause of the attraction of caloric from surrounding bodies. It is merely the remote cause of it by the reduction which it occasions in the temperature. By this change of temperature only are we able to judge of the nature and extent of the change of capacity, and the accuracy of this idea is clearly demonstrated by the uniformity of its correspondence with the change which we find produced in the freezing point of the new compound.

From these considerations we have a right therefore to infer, that the vapour of water, ether, &c. cannot produce cold in the surrounding bodies without having a temperature actually inferior to their's. But we know that the evaporation of these substances produces cold when the temperature of the surrounding bodies is greatly inferior to that which is universally deemed essential to the subsistence of these vapours. We know that, were it possible for the vapours of water, or of ether, to subsist at the temperature here implied, instead of subtracting caloric from such bodies, they would impart their own to them and undergo condensation. We know also that, if it were possible for the vapour of water (for instance) to subsist at a temperature as greatly inferior to that of 212° , it would have a much greater affinity for the caloric of the evaporating water than the water itself: of course that, whilst such vapour was present, the evaporation could not proceed, which is contrary to fact. We are inevitably led to conclude, therefore, that the cold here produced, is not produced by the vapour (understanding the word in its ordinary acceptation) of the subject of the operation. Nor do I conceive how it is to be accounted for, but by ascribing

* The cold in this case is inaccurately said to be the consequence of change of capacity. This change of capacity merely determines the extent to which the proximate cause, the inequality of temperature, shall operate. This distinction, though as obvious, is not sufficiently attended to.

it to that product of the decomposition of the evaporating substance, which, having the least affinity for caloric, is rendered gaseous by the separation of the rest, before it has acquired sufficient caloric to satisfy its resumed capacity.

I might have added several facts of inferior importance to the testimony of those already noticed, but I have thought it sufficient to show that the universal dictate of analogy, as to the combination of caloric with substances whose component parts have different degrees of volatility, is supported by the evidence of the most important phenomena: and that we have reason, therefore, to conclude, that in every such case a resumption more or less complete of the discrete affinities of the constituent parts for caloric must take place, and that in most instances, at least of evaporation, this resumption may be considered as entire, and the decomposition complete. It is certainly not inconceivable that there may be bodies, the constituent affinities of whose parts may be so great as to overcome the tendency of their different affinities for caloric with any heat we can apply. It is not probable, however, that such exist among the substances with whose constitution we are hitherto acquainted.

I am, SIR,

Your's respectfully,

JOSEPH ASTLEY.

Edinburgh, Jan. 31, 1801.

VI.

Account of an Electrometer. By CIT. C. L. CADET.*

THIS author, according to the useful custom of the French writers, gives a short prefatory account of the electrometers of *Dufay* with threads, *Henley* with the quadrant and index, *Canton* by the successive discharge of a jar by sparks, *Lane* by the length of the spark, *De Sauffure* by the balls of *Canton*, and *Coulomb* by the method of torsion; to which he might have added the floating instrument of *Nollet*, the measure of repulsion by weight by *A. Brook*, the extent of electric atmospheres determined by the luminous point, by *Magiotto* and *Van Marum*, the inclination of a thread at a given distance by the same philosophers, and the gold leaf electrometer by *Bennet*. He has purposed to construct an instrument capable of application to the measurement of strong as well as weak electricities, without requiring any attention to friction or weight. Of this I shall give the description and account in his own words:

“ Fig. I. Plate 11. In a glass tube A, 18 or 20 inches long, is inclosed another shorter tube X, sealed at both ends. This tube contains a graduated scale: one of the ends of these

* Abridged from the author's paper in the *Annales de Chimie* XXXVII. 68.

two tubes is cemented in a handle of turned wood, C, by which it is held in the hand; the other end is closed by a brass cap, D; the distance between the extremities of the small tube and that of the large one is filled with red wax; B, B, on the cap D is screwed at pleasure, either a ring E, or a brass hook F. The ring is used for applying the instrument to the ball of a conductor, and the hook when it is hung to a ring: on the cap D is a brass stem G, terminating by a knob. This stem is bended, and the extremity of its knob must be directly beneath the line with which the graduated scale of the small tube commences.

Round the large tube is a brass ring H, half of which extends to the length of twelve or fifteen lines, in the form of a half tube P, applied against the sides of the tube. This gutter serves to mark the degrees, by sliding along the graduated scale by means of a button beneath, I. On the ring H is fixed one of the small electrometers invented by Saussure, K, K, which is surmounted by a stem V, on which stem is fixed at pleasure either a point L, or a ball M, of the same size as that which terminates the stem G, opposite which it is placed. The extremity of this point or ball must be placed immediately over the extremity of the half tube or scale P, and horizontally to the centre of the ball, which terminates the stem G.

At the top of Saussure's electrometer is a small ring N, which serves to connect it with the chain Z, when required.

To explain the use of this instrument by a single experiment, I charge a Leyden jar, till the spontaneous overflowing announces it to be saturated. I then place the ring E on the knob of this bottle, and cause the electrometer of Saussure, armed with its point, to slide towards it. I remark the degree at which the divergence of the thread stream commences, and at that instant I suppress the point, and adapt in its place the ball M. I continue to advance the electrometer of Saussure till the electric pressure of the atmosphere in the jar causes the threads to diverge. I again observe the degrees, replace the point L, and close the shutters of my chamber; I then continue to advance the electrometer till the luminous point appears, which again affords new degrees. Lastly, I replace the ball M, and fix the chain Z to the small ring N; I cause it to communicate with the exterior coating of the jar, and advance the electrometer till the explosion takes place. Then comparing the different degrees, I ascertain the comparative difference between the respective methods.

As soon as these relative proportions have been once accurately ascertained by attentive observations, one of those methods alone will be sufficient for measuring the intensity of electricity, and in fact, if the body intended to be submitted to examination be little charged with the electric fluid, the diverging of the threads by means of the point, will fix the limits of the electric atmosphere: if it be more, the pressure of the atmosphere on the ball M, which is substituted for the point, will indicate this quantity. In short, if the body be loaded with a considerable mass of electric matter, it will be shown by the luminous point. If a Leyden jar, instead of being positively, is negatively electrified, the point

point indicates it at the same time that it measures the electric atmosphere; for instead of a luminous point, a star will be observed upon the ball of the jar, and another at the end of the point.

Let us now apply this electrometer to useful observations :

In order to connect the idea of a determinate quantity of fluid to each degree of the electrometer, it is necessary to compare these degrees with the known quantities. Suppose for instance I have a jar, the coating of which is six inches square; I electrify it till a spontaneous discharge takes place, and remark, by means of Henley's electrometer, at what degree this discharge is effected. I again electrify the jar, till it is nearly saturated, and measuring with my electrometer, I observe, that the luminous point appears for instance, at two degrees; I then say, that when my electrometer, applied to an electrified body, marks two degrees, the body contains six inches square of electricity. I repeat this experiment with a plate of glass, the coating of which is seven, eight, ten, or twelve square inches, and I form a scale of proportion, which is of the greatest utility in accurate experiments.

In endeavouring to ascertain some of these propositions, I made an observation which has convinced me of the utility of my electrometer in discovering the capacity of electric apparatus. Having taken a jar from an electric battery, I electrified it, and measured it with a point which I passed along a string of silk; on observing the distance at which the luminous point appeared, I joined this jar to another of the same size, and imagined that by doubling the quantity of matter, the measure I had taken would also be doubled; on the contrary, however, the latter measure was not more than about one third of the former: I then added a third bottle, and still obtained nearly the same result; whence the following proposition appears to be established; namely, that the extent of the electric atmosphere is in an inverse ratio to the quantity of fluid accumulated. Another observation which I have several times made, on measuring the electric atmosphere of a conductor, is, that the limits of this atmosphere form an elliptic figure around the body, nearly similar to that represented at Fig. 2.

This, doubtless, arises from the electrified body suspended in a chamber, being nearer to the earth than the ceiling; but it would be a curious experiment to measure it at an equal distance from every attracting body, in order to observe whether the fluid has not really a tendency to descend towards the earth, rather than in any other direction. It is my intention to repeat this experiment, as I consider it of great importance to ascertain whether electricity gravitates towards the globe.

From these first attempts, I conceive my electrometer would be well adapted for measuring the absolute capacity of Leyden jars, and also their capacity with regard to their size, or to the quality of the glass of which they are constructed; for the latter, by its greater or less density, absorbs a greater or less quantity of fluid.

VII.

Inquiries relative to the Experiments of Cit. Prevost, of Geneva, on the expansive Force of Odoriferous Emanations, and of Cit. Venturi, of Modena†, upon the Motion of Camphor upon Water. By Dr. F. CARRADORI DEL PRATO‡.*

AS I have been employed perhaps more than any other philosopher, in observing the curious motions of camphor, and all the other oleaginous substances, as well fluid as solid, when cast on the surface of water, and as I flatter myself that I have given a true explanation§ of these phenomena, which had in vain been attempted by Romieu, Volta, Brugnatelli, Lichtenberg, and other respectable philosophers, I was of opinion that it was proper for me to repeat the experiments of Prevost and Venturi on this subject. I have just done this, and find that those of Prevost do not possess that novelty which was at first imagined; but on the contrary, the whole, and particularly those of Venturi, tend still farther to establish the truth of my explanation.

I have proved||, that all the fixed and volatile oils, as well as the resins and concrete volatile oils, for instance, camphor, move upon water in consequence of the attraction which every oil, whether volatile or fixed, has for the surface of the water, by which it is attracted, and compelled to extend itself till the respective attraction of a determinate surface is saturated; and that there are oils, or oily substances, which have a greater attraction than others for the surface of this liquid. This has been precisely confirmed by the new observations of Prevost and Venturi.

The facts related by Cit. Prevost are certainly true, as has been previously remarked by Cit. Brongniart, but the consequences which he conceived to result from them are not equally accurate.

It is true, as Prevost has asserted, that if fragments of a concrete odoriferous substance, for instance small pieces of camphor or particles imbued with an odoriferous liquor, be placed upon a plate, or on a piece of wetted glass, that is to say covered with the slightest film of water, the latter will be immediately seen to retreat from those parts where the substances have fallen or settled, as if it were repelled by some force proceeding from those bodies.

But it is not true that the retreat of the water from the abovementioned small bodies is caused by the atmosphere of odoriferous emanations, which proceed from them with impetuosity, and repel whatever they meet with: but the fact is, as Venturi has already ob-

* Annales de Chimie, Vol. XXI. p. 254.

† Ibid, Vol. XXI, p. 262. See also our Journal, Vol. I. p. 153, and 205.

‡ Translated from the Annales de Chimie XXXVII. 33.

§ Ann. di Chimica e d'Historia natur. di Brugnatelli, tom. v.

|| See my memoir on this subject, in the Opusc. Scelti di Milano. Ann. di Chimic. et Giorn. fisico-medico di Brugnatelli, and particularly Vol. XVI, of the last mentioned work.

served,

served, that the oil of camphor, as well as all the other oils, have a greater attraction than water for the surface of the plate. In fact, Venturi observed, and the same remark was afterwards made by myself, that the part of the plate whence the water has retreated when touched with a small piece of camphor, or any body dipped in volatile oil, remains varnished with a coat of very subtle oil, which is perceptible by the eye*; whence he rightly concluded, that the water retires to obey its affinity of aggregation, which becomes free from the attraction of the surface of the plate as soon as that surface is occupied by the oil; and this effect happens only because this affinity of aggregation or cohesion has become greater than the attraction of the water to the plate. This is so true that the same phenomenon always occurs, whatever oil be employed, whether volatile and odoriferous, or not, such as oil of olives or lintseed. As soon as the wet plate is touched with the finger slightly dipped in one of these oils, the water retreats in a circle, as if it were touched with any odoriferous body.

There is doubtless, as Cit. Venturi has observed, a stronger attraction between the oil and the surface of the plate, than between the latter and the water; which is evidently proved by the observations: let a drop of olive, or any other fixed oil, be dropped on a small plate of porcelane, glass, or any kind of glazed earth; and afterwards pour some drops of water from a certain height, which on uniting together quickly flow like a small torrent over the oil; it will be seen, that notwithstanding the force with which this small torrent strikes the drop of oil, it is not capable of displacing it and carrying it off, but passes over while the oil adheres to the surface of the plate, like a varnish, which the impetuous and continued passage of the torrent cannot remove. It is well known that plates, or other culinary utensils, cannot be cleaned by water when soiled by oil or fat, as it has not force enough to detach the oily particles from their surface, though it can produce this effect with earth, and dissolve extractive substances.

If a small piece of camphor be fixed upon a plate or other vessel, and as much water be added as will slightly cover the camphor, the water, as Prevost has observed, can never attain to a level above the camphor, but forms a small cavity or gulph, in the figure of an inverted cone, in which the summit of the piece of camphor is always perceptible; this evidently proves, according to him, that there proceeds from the odoriferous body an elastic fluid of sufficient force to repel the water that surrounds it, which according to the laws of hydrostatics, ought otherwise to come in contact with and cover the camphor.

Cit. Venturi has likewise observed this phenomenon; but he conceives it to be the effect of an oily fluid which proceeds from the camphor, and prevents the adhesion of the water with the piece of camphor, and with the columns of water that surround it.

For my own part, after the most accurate examination, I am of opinion, that the retreat of the water is neither attributable to the emanation of an elastic fluid, to an odoriferous vapour, as Prevost has supposed, nor to the obstacle that prevents the adhesion of

* Loco citato, p. 207.

water with the camphor, produced by a subtile oil proceeding from the camphor itself; but that it is owing merely to the cohesion between fat or oily bodies and water. Oils possess much adhesion or attraction of surface * with water; but they have no cohesion or affinity of aggregation, nor of composition with that liquid. The water which surrounds oily or fat bodies, such as a piece of camphor, having no degree of cohesion with that substance, appears to retreat from it; that is, by abandoning itself intirely on its own force of aggregation it is drawn off on all sides, which causes a vacuity round the camphor. If it were an elastic odour, or an oily fluid proceeding from the camphor, which produces this phenomenon, the same effect would not take place in bodies which have no smell. But the phenomenon is the same, when instead of camphor, small pieces of white wax or hardened suet are employed, which are substances as every one knows, which have scarcely any smell, containing an oil which is not volatile, and requiring a considerable degree of heat to become fused.

Hence it appears, the experiments of Prevost do not prove that odoriferous bodies are surrounded with an atmosphere possessing a degree of elasticity or power capable of impelling the water which surrounds them; and that it has been falsely imagined that by this means the emanations from odoriferous bodies might be rendered perceptible.

I did not observe, what Prevost says he remarked, that fragments of camphor about the size of a pea, placed on a round piece of gold or silver foil floating upon water gave it motion. The camphor itself is not susceptible of any spontaneous motion, nor can it impart it to light bodies which are on the water, unless it immediately touches that liquid. It is the same with regard to the oils, whether volatile or fixed; they cannot impart the least degree of motion to the lightest body that floats upon water unless they touch it. I have made several trials, by putting drops of ethereal or volatile oils on small pieces of gold or silver leaf that floated upon water. The slightest impression would have caused them to move, yet I never observed the least motion in any of them. Venturi also had the same result in an experiment with burning or heated camphor, placed on a piece of cork which floated upon water. When the camphor came in contact with the fluid, it imparted a rapid motion to its support, which, on the contrary, became motionless when the water was not touched by the camphor. Hence he concluded, that the motion in question does not depend on the projection of volatile particles proceeding from the camphor, but that it is the water alone which excites it by its action.

Prevost and Venturi remark, that if the surface of the water be touched with oil of olives, or any other volatile oil or odoriferous fluid, when the camphor begins to move, the motions immediately cease. This only tends to confirm the explanation I have given of the motion of the camphor and oleaginous bodies on the surface of water. The camphor derives its motion only from the expansion of an oil, which is attracted by the surface.

* See my explanation in *Ann. di Chim. Italiani*, tom. V.

of the water *. The reason therefore why a drop of oil, whether fixed or volatile, stops the motion of the camphor, is, that these oils are more strongly attracted by the surface of the water, than by that of the camphor; hence they adhere to that surface, and by covering it prevent the effect of its attraction for the oil of the camphor, and consequently prevent the expansion of that fluid †. This property of preventing the motion of the camphor is not peculiar to the oils; all the cereal and leguminous farinæ, &c. the juices of the tithymalæ, and other lacteous plants, which, as I have observed in the memoir before-mentioned, have a great affinity for the surface of the water, are invested with similar properties.

Cit. Venturi has nearly discovered the real explanation of the phenomenon. Having observed that a small quantity of oil stopped the motion of camphor in a basin of water, he concluded that this oil, which extends over the water like an imperceptible film, is the cause of the suspension of motion in the camphor, because, by occupying the surface of the water, it prevents the camphor from expanding. Whence he seems to be of opinion, that the motion of pieces of camphor upon water proceed from the expansion of an oily substance from the camphor upon the water itself. This is the explanation which I have given of the above-mentioned, and other similar phenomena, in several memoirs published previous to the year 1794.

Cit. Venturi however, appears in some degree to follow the opinion of Prevost, since he asserts that the volatility and odoriferous property of camphor are not qualities requisite to produce the revolving motions which this substance exhibits when cut in small pieces and cast upon water; but that its volatility is nevertheless necessary for the continuance of such revolutions.

The motions of camphor, as I have proved in the memoir so often quoted, and which is inserted in the xxth vol. of the "*Opusculs choisis de Milan*," depend on two causes, viz. the extension of an oily fluid over the surface of the water, and on the rapid operation (evaporation) of this fluid itself.

When camphor cut in small pieces is thrown on the surface of the water, the instant they touch this fluid they begin to move in a circular direction, and run with rapidity in different directions; but shortly afterwards their motions decrease, and the particles collect in small heaps; which, though they have not an absolute, have nevertheless what may be called an intestinal motion. All this takes place in consequence of the consumption of the particles of camphor which form the small heaps; for according to the former remarks of *Lichtenberg*, as they necessarily change their figure, they are compelled almost every instant, being subject to follow the attraction of the mass, which varies the contacts according to their diversity of figure. The water, by the attractive power of its surface causes the extension of the volatile oil of the camphor. This oil, by exposing a great su-

* Opusc. Scelti di Milano, tom. XX.

† Vide my Memoir "*Sulle attrazion di superficie*," Journal des Phys. et Ann. di Chimic. Italiani, tom. XVII.

pericies to the air, evaporates more speedily, and as its evaporation is in proportion to its extent upon the water, the reason is evident why the camphor is so speedily consumed, and why by this rapid consumption the particles are induced to change their figure.

It is true that the oil camphor, which is extended over the water, gradually evaporates or entirely volatilises. Of this I have been clearly convinced by the following experiment : after two pieces of camphor which had been in motion on the surface of water in a glass were consumed, I exposed the glass to the air, that the oily film of oil camphor which covered the surface might evaporate; four hours afterwards I put in other pieces, but did not observe the accustomed winding motions; I soon removed them, and at the expiration of four hours I again threw in fresh pieces; and the motions took place as usual. Hence it appears, that after four hours the oil of the camphor had not totally evaporated, though in eight hours it had disappeared, and the water remained perfectly clear.

Venturi, however, was well acquainted with the attraction that exists between oil and the surface of water; since he observes that a drop of oil, if it had no affinity with the surface of water, would remain upon it without extending; it may therefore be said, that an attraction exists between the substance of oil and the surface of water.

The experiments made by Venturi with small columns or sticks of camphor, plunged half their height perpendicularly in water, and cut horizontally at the level of that fluid, in the space of twenty-four hours, only proves that the greatest solution of camphor is effected exactly where it touches the surface of the water; and consequently they tend to confirm my conclusion, that camphor contains an oil which expands over the surface of water in consequence of an attraction between it and this surface; and that it is by such extension that it more speedily evaporates. There is not, as he imagined, a greater activity exerted upon the camphor at the points where the air and water meet in contact with this substance, but only where the camphor touches the surface of the water. Air only conduces to this influence upon the phenomena, in proportion as it favours the evaporation of the oil of camphor which is extended over the surface of the water. I am well convinced that air here has no influence, and that the whole effect ought to be attributed to the union of the surface of the water.

But it appears that Venturi has at length discovered his error, since he says, he observed that the solution of camphor at the surface of water takes place the quicker the more that surface is extended. For in small vessels the cutting of the columns is not so quickly effected.

Prevost has also confirmed the attraction of the surface of water; as he observed that camphor when in a humid air, or submersed in water, does not undergo so much loss or consumption, as it suffers when it immediately touches the water; but he attributes this to the water, which favours the evolution of the odoriferous fluid.

The rapid motion of odoriferous bodies upon water, and particularly that of camphor, is prevented after some time, as Cit. Prevost has observed, in consequence of the attraction of the surface of the water for the oil which expands being diminished: nevertheless, the small pieces of camphor which collect on the water after their revolving motion has ceased,
always

always retain a slight degree of motion, by which they resemble groupes of capillary eels, that move spontaneously, till they are entirely consumed by evaporation: this motion depends on the slow expansion of the volatile oil of camphor which takes place at the surface of the water, in proportion as that which remains is evaporated, as we have already mentioned.

At first I entertained doubts whether the motions of the camphor upon water were suspended by the expansion of olive oil, or any other oleaginous substance, merely because that of the camphor, being more dense, did not possess the faculty of expanding as quick as the liquid oils. To elucidate this point, I made the following experiments: I melted a small quantity of camphor in a spoon by the flame of a candle, and poured it upon hot water, on the surface of which a drop of olive had previously expanded: in its melted state it was not at all capable of extending, and expelling the olive oil; still less of giving motion to farinaceous matter. I next threw some pieces of the inflamed camphor on the surface of cold water; and they moved extremely well. As I supposed that it was the heat of the inflammation, which by melting the oil of the camphor obliged it to continue to expand, I poured on this water some drops of olive oil; the motion of the camphor then immediately ceased, and did not recommence, though the substance continued to burn. Hence it must with certainty be concluded, that the oil of camphor has less attraction of surface for water than olive oil, or several other oleaginous substances, such as the flour of wheat.

Camphor, well heated, and fuming, as Venturi has observed, being brought near very light bodies floating on the surface of water, such, for instance, as pieces of gold or silver leaf, exerts upon them a repulsive force, exactly the same as when it is thrown upon cold water; and, according to him, this effect is owing to the elastic fluid, which strikes against those bodies: this notion also occurred to Prevost. For my own part, I think this effect is produced only by an oil which expands upon the water: the oil of camphor, reduced to vapour by heat, when it comes in contact with water, expands on the surface, and thus imparts a motion to the bodies it meets with, and seems to repel them. In fact, if we attentively observe the surface of the water around the floating body which is moved, it will be seen to be covered with a small very thin and whitish film (of varnish) which is, doubtless, a proof that the oil of the camphor, when volatilised, is extended over the water, and by cooling deposits a fine incrustation.

It is not, therefore, proved by any of these experiments that the atmosphere, which is formed by the odour of the bodies, possesses, as Prevost pretends, an expansive power capable of being rendered perceptible. Those bodies which are most effectually odorant, that is to say, surrounded, like the others, by an atmosphere of odoriferous emanations, but which are not mixed with a volatile oil, do not cause any repulsion in bodies floating in water, however near they may be brought to each other.

And if the mere emanation and projection of an elastic odoriferous fluid were the only causes of the motions of camphor and other odoriferous bodies by water, these motions would take place on throwing in any one of such bodies; but if, for example, even the smallest piece of camphor be dropped upon water with a confined surface, the motions are not produced; it is therefore evident that they are caused by the surface of the water alone.

VIII.

Account of the original Invention of the circular Instrument for performing Calculations on the Principle of Gunter's Rule made by Clairault le Père, and communicated to the Royal Academy of Sciences in the Year 1727.* (W. N.)

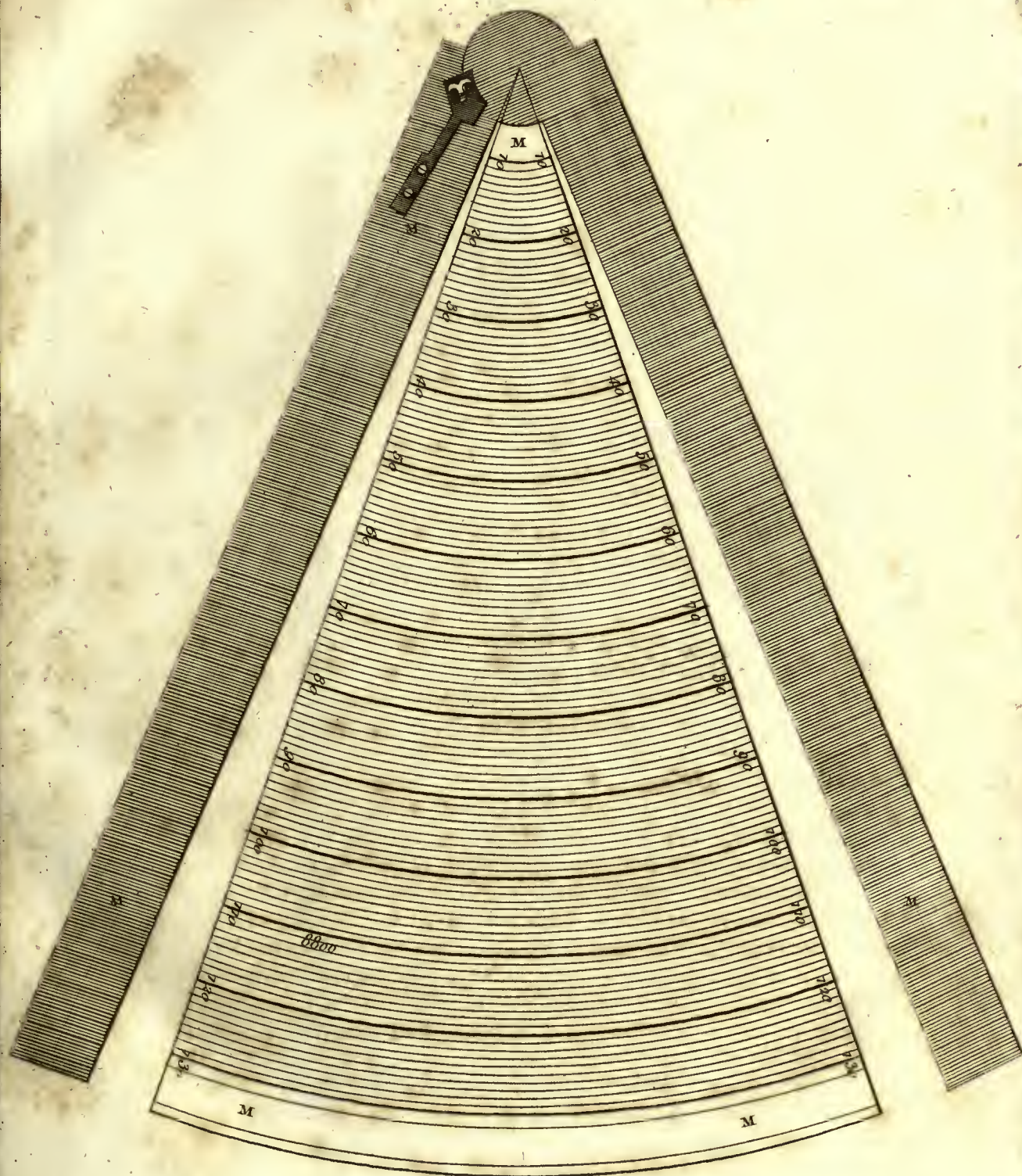
IN the first volume of this Journal, page 372, I gave an account, with drawings, of a spiral instrument consisting of a combination of the sector and Gunter's rule, equivalent to an ordinary rule of forty feet in length, which had been communicated by me to the Royal Society about thirteen years ago, as my own invention. In the paper here referred to, I did justice to George Adams the father, who, as I afterwards discovered, had contrived and made a very powerful instrument of the same kind in 1748, since presented to me by his son. The same principles of candour and justice call upon me at present to ascribe the original thought, as well as the actual mechanical construction, to the elder Clairault; of whose engraving, published twenty-one years before the date of Adams's manuscript, I have here given a copy. An abridged account of his paper will shew that he was well aware of the properties and advantages of this apparatus.

He begins by observing that the best expedients for the solution of right lined triangles are reducible to linear construction, or logarithmic calculations; that the first, by means of the sector, requires much skill, to produce a moderate degree of accuracy: while the latter demands a certain quantity of research and computation, which render it desirable to simplify the process. With this intention he proposed to unite these two excellent discoveries which he performed by placing all the logarithms distinctly on the surface of a circle of twenty-one inches in diameter, upon concentric circumferences; marking not only the natural numbers as far as ten thousand, but also the fines of the degrees and minutes. The figure in plate 1 represents a twelfth part of the circular instrument, and the external shaded branches denote the sector between which the intervals are measured. In the year 1716, the author executed his first system on a square of one foot, filled with parallel lines, constituting altogether a rule of 1500 French feet: and it was not till the year 1720 that he thought of the spiral form. But as this set of lines, when numerous, presented some difficulties, principally, as I suppose, from the difficulty of marking and counting the intervals upon the blades of the sector, he gave the preference to concentric circles. I need not enter into the minute particulars of the construction, because this has been already done in our Journal, and in the Philosophical Transactions. He does not overlook the advantage (which also occurred to me) of this structure that it affords by the enlargement of the external arcs, a remedy for the smallness of the differences in certain parts of the logarithmic lines.

The learned writer concludes his paper by giving some numerical examples of its use and application.

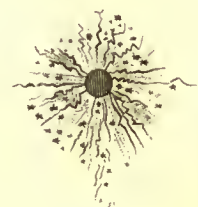
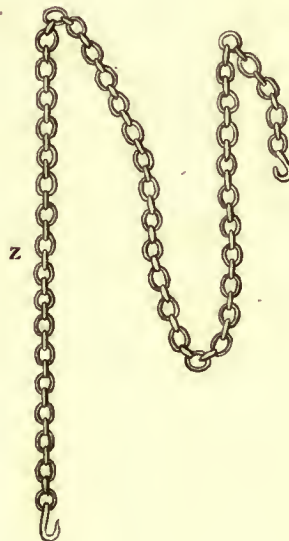
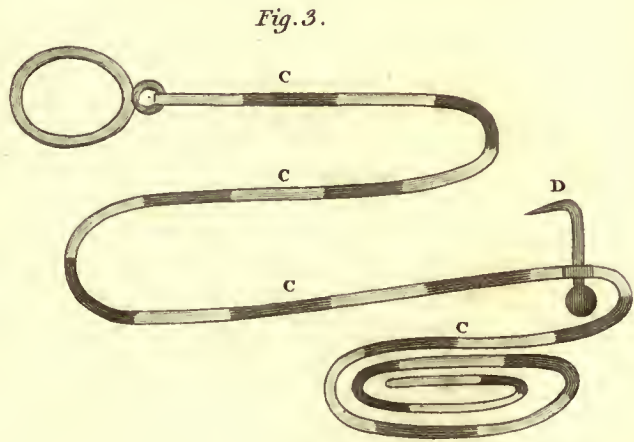
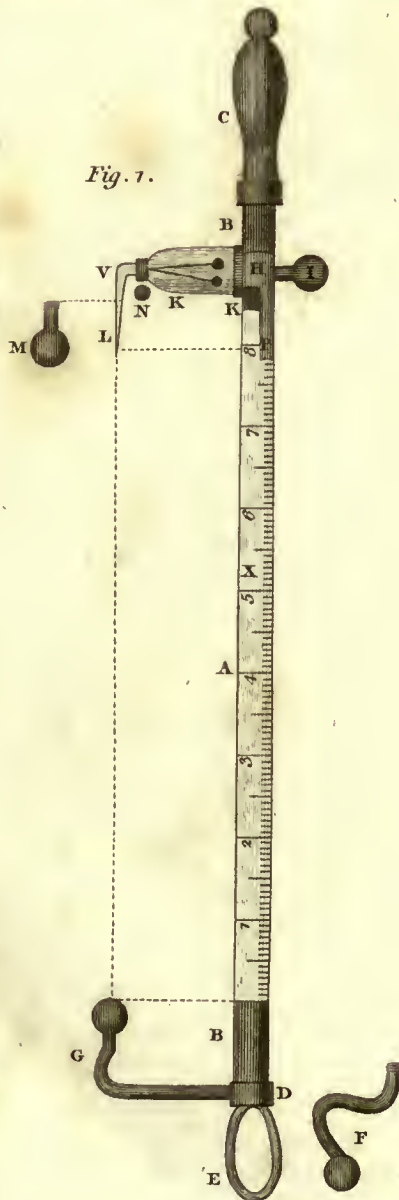
* *Machines approuvées par l'Acad. Roy. tome V.*

M. Clairaut's Instrument for Logarithmic Computation.





Electrometer of Cit. Cadet





A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

MAY, 1801.

ARTICLE I.

Observations and Experiments on the Galvanic Power. By a Correspondent.

TO MR. NICHOLSON.

SIR,

Edinburgh.

THE readiness with which you admitted a former communication of mine (notwithstanding that its deficiencies must have been apparent to your acuteness, and that there are now several parts of it which later experiments would induce me to correct or explain), has encouraged me to lay before you some phenomena that I have observed in the interesting science of Galvanism.

I began my inquiries on that subject almost immediately after the first publication of the account of Signor Volta's Pile in your Journal, in concert with a friend, who, to the experience of years, added all the ardour of youth in the pursuit of science, but whose life was shortened by an unfortunate accident in a vigorous old age: I premise this account of my own experiments, by rescuing from oblivion the few observations that his time and health would allow him to make; and which will come with no small interest to the world, when it is known they were those of the intimate friend of a Franklin and of a Black.

VOL. V.—MAY 1801.

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His

His first attempts led him to compare Galvanism with the old electricity; and he was decidedly of opinion, that there was a sufficient distinction betwixt them to entitle the former to the rank at least of a new species; this observation my own experience has every day confirmed, especially since I have been able to give so very considerable a degree of strength to the pile as I have latterly done.

He attempted to use resin as a lining for a white iron case to the pile, in which, after standing for a short period, its power stopped; and upon examining the pieces he found the resin completely dissolved into a substance resembling turpentine, and forcibly drawn betwixt the silver and zinc; this shows a violence of action which has never as yet been observed in Galvanism, and may serve as a caution to others against using resinous substances with the pile, at the same time that it opens a field of inquiry with regard to its effects on that matter.

His next and most important observation was made on a most singular phenomenon, which he perceived on the sides of the silver and zinc pieces which were in immediate contact, when strong pasteboard, moistened with a solution of common salt, was used for setting up the pile: the pieces of pasteboard were of a polygonal form, and smaller than the metallic pieces; their figure was exactly stamped in a black colour, not on the sides of the pieces in contact with them, but on the dry metallic faces which were in contact with metal only; this very singular appearance, which I had frequent opportunities of examining, conveys an idea that something, circumscribed in its action by the figure of the moistened disks, and capable of penetrating through the pores of the metals and of oxidating them, formed a constituent part of the Galvanic fluid.

He also observed, what might be readily expected, that people advanced in years felt the small shocks of Galvanism less readily than younger persons; but what was singular, they felt the strong shocks more severely; this may throw some light on the nervous fluid, and the change that it undergoes by age.

He found that when the experiment of decomposing water was made in an open glass, that the water, although kept for a week after, unconnected with the pile, and a sufficient quantity of salt had been added, to prevent all symptoms of putrefaction, yet continued to emit a gas, and a mucous matter was precipitated from it, of a whitish colour; this seems to have some connection with an extraordinary liquor, procured by the ingenious Dr. Moyse, by evaporating the wastings of the pile; but the account of which I by no means wish to anticipate, as it is to be hoped that the learned Doctor will favour the world with it himself.

You will no doubt regret that so truly philosophic an observer should be lost to mankind, and had you known the individual, you would have found that the powers of his mind could only have been equalled by the virtues of his heart.

I now, with diffidence, subjoin an account of my own experiments.

Being acquainted by observation, and by the experience of others, with the Galvanic powers of black lead, I attempted to form a range of plumbago crucibles, into a Galvanic apparatus,

apparatus, which has the advantage of cheapness, and considerable power, and would no doubt answer very well, if a way could be discovered for preventing the moisture from penetrating through the pots, though I have not been able to effect it by any mode of coating them: in case anybody should be inclined to try this method, I have annexed a drawing of the manner in which the pots are connected, at the same time observing that the zinc pieces ought to be soldered to the brass wire. If ever Galvanism can be applied to the arts, by procuring alkali in an economical process, the above bids fairer to attain that end than any thing I have yet met with, as the pots are in a short time covered with an efflorescence of soda when common salt is used, which might be collected tolerably pure from their outflows.

In this range the zinc became the gaseous, and the plumbago the oxidating side, which shows that it is as far beyond zinc, as zinc is beyond silver, in the arrangement of Galvanic oxidating power, though there is no reason to believe that the plumbago undergoes any change itself. When connected with a bent syphon, filled with distilled water, by either silver, brass, or iron wires (even though the bend was filled with quicksilver), both sides soon changed the reddened litmus paper to the purple, though the plumbago side restored it to be a perfect blue; showing that a much greater quantity of alkali was formed on that side than on the other.

The only apparently electric phenomenon, independent of the shock, that I could observe by this range, was that the powder of the plumbago assumed the form of little stars on the surface of the water, which some people might be inclined to think proceeded from a state of positive electricity.

Having lately discovered a mode of increasing prodigiously the powers of the pile, I have laid aside the cumbersome and bulky apparatus of the range, and cannot recommend it for any thing but its cheapness, and the possibility of being satisfied at a moderate expence, by means of the larger sized crucibles, of the proportion that bulk or surface bear to alternated numbers in Galvanic power.

I also annex a drawing of the box, in which I enclose my pile, and which I can recommend for convenience and portability; the improvement that I have made in the pile is on the matter by which the pieces are put together, to which I was led by theory.

I took a pound of pipe clay, an ounce of plumbago powder, an ounce of black oxide of manganese, and two ounces of common salt, and made them into a paste with water; in the use of clay I took advantage of the observations of Dr. Moysse.

This mixture gave 126 pairs of silver and zinc of the half-crown size, a power beyond any thing I have yet heard described, and gave the spark most vividly, in a room where the sun was shining, either from a bit of common coak, or the point of a needle; but even when two pieces of metal were held in the hand, though perfectly dry, and the wires touched by them, a shock was felt much greater than that procured from a very considerable number of pieces with the assistance of moisture, set up in the common way. When the pile begins to weaken a little, if one of the wires is allowed to vibrate on the piece of metal

in one hand, and the other wire held by the other, the sensation of this dry shock brings a complete recollection of what is commonly called the sleeping of a limb, viz. that prickling that is felt after the limb has been long in a constrained position, and when, I believe, medical people suppose there has been a pressure on a nerve.

I am almost inclined to believe, that the natural sensation proceeds from the Galvanic fluid in the human system attempting to restore its equilibrium by repeated shocks. A pile in this very strong state gives no signs of electricity by a very delicate electrometer (which I used to the exclusion of the modes of accumulating electricity, on which I think little dependence can be placed), and no effect could be excited, even though the pile was insulated, and the shock taken through the metallic arm that supported the electrometer, by a person on an insulating stool.

I also attempted to charge a Leyden vial in the way * mentioned by you, as used successfully by Mr. Cruickshanks, viz. connecting one end of the pile with the outside, and the other with the inside of the jar. It strikes me that it is impossible a jar would be charged in that manner, as the two sides, in that case, are connected by conducting substances, the very mode used for discharging a jar; if I have mistaken your meaning, or that of Mr. Cruickshanks, I beg you will correct me. I also connected both wires with the outside, and both with the inside, and even the silver wire of one pile, and the zinc wire of another, alternately with the internal and external sides, without producing any effect.

I then attempted to charge the vial through the insulated pile, connecting the wires by turns with the conductor of an electrical machine and the inside of the jar. By comparing the change produced by a definite number of the revolutions of the machine, when attached and unattached to the pile, and when the silver and zinc wire were alternated, in the manner above described, no difference could be perceived either in the shock or size and length of the spark: also when the jar was placed between the conductor, and the insulated pile, no difference could be perceived by taking the shock or the spark through the pile.

These experiments were often repeated with the same results; and I think they show clearly that the charge is neither accelerated nor retarded by the pretended *plus* and *minus* of the pile.

I have several times observed, and wish it may be attended to by others, that when women and men form a part of the Galvanic circuit, that the women feel almost the whole

* Your words are---“ That Mr. Cruickshanks, of Woolwich, has charged a large jar so as to give a shock merely by placing the pile between its coatings.” This I suppose was a private communication, as I have in vain looked over Mr. C.’s own papers for the fact.---C.

The communication was made in conversation. I think the arguments and inductions at p. 243-- 245 of our IVth volume remove many of the difficulties of theory, arising from the low intensity of the power; but not this of the actual shock. I have charged talc as well as thin silk unequivocally; but expect soon to repeat the experiments with a higher power.---N.

shock, when the men feel little or nothing; this even happens when only two persons are in that situation, and with whatever side of the pile they may be connected, and when one woman stood between two men.

I believe we owe to the learned professor of botany, in this university, the observation, that plants are not conductors of the Galvanic influence; a most striking distinction betwixt it and the electric fluid: I connected the pile, in its strongest state, with the stalk of a live and herbaceous plant, the stem of which was even moistened with salt water. The shock could not be procured from any part of the plant, however near to the wire, and one of its leaves applied to the wire, sufficiently interrupted the Galvanic power *

The Galvanic spark procured from charcoal, has a very different appearance from the electric received in the same way; the former being of a red colour, and resting upon the eye, whilst the latter, notwithstanding its superior velocity, can be distinctly perceived, to have the blueish cast of burning sulphur.

The intention of this communication will be fully answered, next to preserving a memorial of a person deservedly dear to all his friends: if I shall be found to have added a drop to the ocean of science, or to have facilitated in the smallest degree, the pursuits of others on the same subject,

I am, SIR,

with great respect,

A FRIEND TO THE EXPERIMENTAL RESEARCH OF TRUTH.

Figure 1st. A A represents the black lead crucibles, and as a range for Galvanic experiments. B B the connecting wires, to which pieces of zinc, as C, are attached by folder.

Figure 2d. is the pile box, of which D D D D are the spaces, in which the piles stand. The shelf E E E, moveable easily for two thirds of the height of the box, by means of the rod F F, which passes through the top of the box, and may be fastened by the flat wedge G; the rod is preferable to a screw, being less hurt by moisture.

Figure 3 is the shelf E, taken out to shew the manner in which it is formed.

Fig. 4, is one of the divisions of the box which are expressed by H H, in Fig. 2, I being cut out for the shelf to run in.

N. B. A front piece shuts the box, by sliding in a groove, on each side from the top downwards, which is sloped away at the edges, and can be thrust under the base of the box; which gives it an inclination backwards, that facilitates the building of the pile to any height. I have lately much improved the box, by lining it with oiled silk, with a view to prevent the wood from absorbing moisture.

* I have undoubted information that a portion of the aloe-plant, which was placed for a night in the connecting line of a pile of 36 half crowns and zincs was killed by its action, the other parts remaining unhurt.---N.

Third.

II.

Third Communication on the Trains of Watches. By the Rev. W. PEARSON.

TO MR. NICHOLSON.

S I R,

WHEN I transmitted to you the two papers, contained in the third Volume of your Journal, respecting the philosophical uses and trains of watches, I had not an opportunity of satisfying myself with regard to one material consideration in the construction of those delicate machines; namely, whether or not the diameters of the wheels ought to be successively diminished from the center wheel to the end of the train, in proportion as the maintaining power becomes exhausted; *four* beats in the second were then fixed upon, for the new trains recommended, for no other reason but because this seemed a convenient number to be divided and subdivided without a remainder, and was also very nearly equal to what one of the watches, under examination at that time, was governed by: since, however, I came to reside near London, I have inquired of some eminent watchmakers what number of beats in a second, is deemed in practice most desirable for preserving a steady equable motion in the going of a watch, and the information I have received, particularly from Mr. Hes, of St. Martin's Court, is, that about *five* is found to be the best number of any: now as the number of beats of a watch in a second has been shown to depend upon what may be called the third, or last portion of a train, (in a watch which shews seconds) with the contrate wheel, its pinion, and the balance wheel; and as five beats in a second require higher numbers, in either one or both of these wheels, than four do, the inference is, that, allowing 15 to be the best practical number for a balance wheel, the contrate wheel must necessarily be larger than I have before given it, in order to produce the desired effect of five beats in each second.

Hence arises the necessity of my troubling you with a third communication on this subject, which, I am persuaded, you will readily lay before the public, not only because watches are in the hands of every reader, but also because you are aware, that they can be made to measure fractional parts of a second for as little expence, as they are generally constructed now that no attention is paid to the exact measure of a beat.

In order to make a watch beat exactly five times in each second, whilst 15 is the number of the balance wheel, the contrate wheel and its pinion must be to each other in the ratio of * 10 to 1; so that if 6 be the pinion, 60 will be the wheel; if 7 then 70; if 8, 80, &c. and as experience seems to point out the expediency of retaining 15 as the most

† When the beats of a watch are four per second, the ratio between the contrate wheel and its pinion is only as 8 to 1.

eligible number for the balance wheel, any one of the above pairs of wheels and pinions may be adopted, that the artist finds his other work will admit of:—what appears to me, on considering the subject in a practical point of view, as the most eligible train, out of the many that have been suggested, is $\frac{64}{8} \times \frac{60}{8} \times \frac{60 \times 15 \times 2}{6} \div 3600 = 5$, which may be otherwise, perhaps more intelligibly, expressed thus :

64 Center wheel

Pin. 8—60 Third wheel

Pin. 8—60 Contrate wheel

Pin. 6—15 Balance wheel

2 Palettes.

Besides the advantage that this train, or one of a similar value, has over those before specified, in point of producing a steady equable motion, it has the further recommendation of being capable of being counted *decimally* without any trouble ; for when any number of beats are reduced into seconds by the divisor 5, the remainder *doubled* will, in every instance, be so many tenths of a second over, and will be an even or odd number, accordingly as the termination of counting by the beats coincides with a beat, or falls in the interval between two ; in the latter of which cases there will be, besides the beats remaining after division, an half beat to be also doubled : thus, for example, in 132 beats of a watch, which beats five times in a second. there are $26\frac{2}{5}$ or 26,4 seconds, but in $132\frac{1}{2}$ there are $26\frac{2\frac{1}{2}}{5}$ or 26,5.

When your readers consider what expence has been incurred in improving watches by the introduction of detached escapements, compensation balances, &c. whilst notwithstanding a *second* is the smallest portion of time they are hitherto intended to measure, they will, doubtless, see the propriety of making choice of such numbers in the construction of those portable chronometers, as will not only tend to improve their rate of going, but will render them, without any additional expence, capable of subdividing the second into exact *tenths*, and consequently of becoming admirably subservient to a great variety of philosophical researches, in which a minute attention to time is indispensably necessary.

In a watch constructed to beat five times in a second, the main-spring must be somewhat stronger than in one which beats only four times, because the contrate wheel, which is a *driver*, has its diameter enlarged ; but what appears to me to be the greatest practical objection to this construction, is that which applies to the destructibility of the last pinion ; by a careful examination of the works of an old watch, it will appear, that the pinions wear out before the wheels, and first of all that pinion which is placed on the arbor or axis of the balance wheel, by reason of the number of its rotations : on this account, particularly where the ratio between the contrate wheel and pinion in question is so high as 10 to 1, the *temper* of the pinion ought to be well attended to, which is usually left too *soft* by the
the

the workmen; partly, I suppose, because the pivots are afterwards more expeditiously finished, and partly because a watch is thereby liable to be the sooner unserviceable.

There is, however, one recommendation more of a large contrate wheel, which I shall just mention before I conclude the subject: as the arbors of the contrate and balance wheels of watches of a common construction, placed one horizontally, and the other perpendicularly, they are necessarily laid at one side of each other, so as just to escape touching as they cross, by reason of which position an obliquity is occasioned in the direction of their action, which is the greater the smaller the contrate wheel is, and *vice versa*; and which frequently occasions the stopping of the watch: an enlarged contrate wheel will consequently greatly diminish this cause of imperfection, and will therefore constitute an additional improvement.

I am, SIR,

Your's respectfully,

W. PEARSON.

Parson's Green, April 13, 1801.

III.

A Method of measuring a Ship's Way at Sea, with Observations on other Branches of the Art of Navigation. By JOHN COOKE, Esq. M. R. I. A. Communicated by the Author.

LET a tube with a few inches of one end bended to a right angle, be affixed to a ship, in such a manner that the longer part may enter through the ship's bottom below the water line, and be fastened in a position parallel to the mast, and so that the recurved end shall be immerfed in the water in a position parallel to the keel, with its orifice pointed towards the ship's head. Let the upright part of this tube be furnished with a small spouting aperture within the ship, at the level of the external water; then suppose it to move steadily through smooth water in the direction of its length, the impulse of the stream on this orifice will be as the square of the ship's velocity (see note A). And since the column of water thrown up in the erect tube by this force must be in equilibrio with it, the perpendicular height of this column of water will be as the square of the ship's velocity also; in which case the water will flow from the aperture into the ship, while the vessel moves forward, and the quantity discharged will be in a subduplicate ratio of the perpendicular altitude of the column in the erect tube; that is in the simple ratio of the ship's velocity. If therefore the quantity discharged through this aperture in passing over a mile be observed, the actual distance run at any time may be found, if the water ejected be preserved and measured (see note B).

But the pitching of a vessel at sea, and the motion of lee way, would cause the stream to fall obliquely on the orifice, and consequently affect the pressure and alter the ratio
which

which exists between it and the ship's motion in direct progress. To prevent this, a short tube is to be fitted to the extremity of the immersed tube, in such a manner as to be capable of moving freely in a vertical, in an horizontal, or in any intermediate arch (which may be effected by a joint consisting of a portion of a spherical shell containing a similar shell) to be furnished with vanes extending behind the joint, which are to be placed in such a manner, that the stream acting on them may point the orifice so as to meet it perpendicularly; but a ship at sea is often deflected from her path into a curvilinear track by the figure of the waves; and since the stream is caused by the relative motion of this water to the ship's motion, which may always be considered as in the direction of the keel, it must meet the orifice of the instrument in the direction of the keel, (see note H) that is perpendicularly, and therefore the water delivered through the spouting aperture must represent the undulating curve through which the ship moves, instead of its absciss, or true distance, (see note C) which might sometimes differ considerably from it; for which reason the following method of correcting it becomes necessary. Let a barometer made narrow in the middle to check the sudden motions of the mercury, be placed against a bulk-head, or transverse partition of the ship, and so loaded and hung that when the ship heels it may swing in an arch described on the surface of this partition, so as not to partake of this lateral inclination (which is not difficult to accomplish, the motion of heeling being neither very sudden nor fluctuating) but in such a manner as to receive the entire effect of the pitching motion which it is intended to represent; then if the curve which the ship describes be supposed to be divided into indefinitely small equal right lines, any one of them will be to the portion of horizontal distance which it overhangs, as radius to the cosine of the angle which it forms with the horizon; but the length of the column of mercury in the inclined position of the barometer, is to its length in an erect position as radius to the cosine of the same angle; therefore the length of the column of mercury at any time will be to its perpendicular height, as the rectilinear portion of the path which the ship shall be then describing to true distance; and since these rectilinear portions are equal, and passed through in equal times, the mean of them obtained by the narrowed tube will represent them all, therefore the length of the column of mercury marked on the scale of the barometer will always be to its perpendicular height as the sum of these rectilinear spaces; that is, as the curved path of the ship represented by the water discharged by the instrument, is to the sum of the horizontal spaces overhung by them; that is the true horizontal distance, from which data the instrumental distance may be easily corrected at any time. But it is to be observed, that the height of the mercury in a perpendicular position is variable, on account of the difference in the weight of the atmosphere at different times; therefore when it is required, the barometer must be taken from its place, and held carefully by the hand for a few minutes in an upright position, until the mercury ceases to descend, when it will shew the true perpendicular height necessary to this experiment, and at the same time the barometrical state of the atmosphere (so useful at sea) more accurately than any suspended barometer.

The agitations of the sea affect also that part of the instrument which lies within the ship, and it remains to be considered what consequences arise therefrom. Heeling, or an inclination of the vessel to one side, and pitching, or the librations of it fore and aft recline the upright tube, and thereby lengthen the column of water contained; but since the discharge of water is affected by *perpendicular* height only, this is but an apparent cause of error; the same pressure at the immersed orifice will elevate the water to the same *perpendicular* height whatever the length of the column may be, and therefore the velocity of the effluent water will be the same with the same degree of pressure whether the tube be erect or inclined; plunging, or the ascending and descending motion, occasioned by the waves which alternately elevate and depress a vessel in the sea, by altering the weight on the orifice increase or diminish the efflux of water from the tube, to remedy which a part of the upright tube below the water line is made narrow, in the manner hereinafter described, whereby the ascent and descent of the contained water will be rendered gradual, so that its altitude may become the mean of several alternations of pressure; and since the waves succeed each other at equal intervals, and of equal dimensions for a considerable time together, this mean height will be sufficiently steady and exact; (see note D) and though this motion and pitching may seem to disturb the operation of the spouting aperture, by raising and sinking it above or below the plane of the water line, it occasions equal and opposite errors thereby, which remedy each other, and narrowing the tube is really useful only to procure a more equable flow from the aperture; but heeling, which is a more durable position, has a tendency to raise the spouting orifice above the plane of floatation; however, this elevation is so small, that the error arising from it is insensible.

Some mechanical difficulties present themselves in applying this instrument to a ship, and in adapting it to the purposes of navigation; first it may be thought unsafe to make a hole in the ship's bottom to receive the tube, but the experiment has been tried to supply fire engines with water on ship-board, and no inconvenience arose from the practice when the receiving pipe was closely fitted to the perforation. It might also be passed, if thought preferable, through a hole above the water line, and the necessary curvature given without the ship.

Secondly, it appears from calculation, that the water in the erect tube would sometimes ascend so high, as to require a tube inconveniently long to contain it; but if it be cut off any where above the spouting aperture, and if the upper orifice be closely stopped, it will answer the purpose as well as before, because the pressure which the stopper receives, is that which would support the column of water cut off, and therefore the pressure which this stopper gives, is that which would arise from the weight of the column cut off. Hence the pressure of the water within the tube continuing the same, the rate of discharge will remain unaltered by shortening it.

Thirdly, it is difficult to ascertain the true mean level of the sea when the ship is in motion, which may often be requisite; in this case, if a flat piece of metal be fixed on the

top.

top of a pole, and held out of the ship so as to defend the external orifice from the percussion of the water, the contained water will sink to the true mean level, and the position of the spouting aperture, if fixed in a sliding plate, may be adjusted thereby *

Fourthly, if dirt or weeds get into the instrument, it may be cleared by forcing out the contained water by means of the plunger hereafter described; or if this plunger and the stopper be drawn out, the water will spout up forcibly in sailing at a moderate rate, and thus remove the obstruction.

The advantages expected from this instrument are, that it proposes a *perpetual* measure, which adapts itself to every alteration of the ship's motion, and therefore corrects that error arising from the log, the use of which supposes the ship's motion to be uniform during the intervals of experiment, which is highly improbable.

And secondly, that the instrument being sunk to a considerable depth, will not be affected by superficial currents occasioned by wind, to which the log is exposed.

The causes of error which affect this instrument, are the same which attend the methods of navigation in general use.

The error of yawing is that which arises from unsteady steering, and will deflect the orifice from the true position.

If it be admitted, that the generality of seamen steer so as to confine the yaws within certain limits, the true line of motion may be considered as a serpentine line, which has a fixed ratio to the right line which joins its extremities in all cases; but if the quantity of discharged water which represents a mile, be found from a mean of several experiments made at sea (which experiments are of course affected by yawing) it follows that the standard measure thus established needs no correction, the effect of yawing being proportionate, and being allowed for in ascertaining the standard quantity.

The error of *lee-way* may be corrected, if a metal rod be extended along the axis of the upright tube, and passing through its bottom into the water below it, it should be capable of being turned round in any direction by the stream acting on a vane like that of a weathercock, which is to be annexed to it below the tube; on the top of the tube is to be placed a circular plate engraved with the points of the compass, through the center of which this rod is to pass, and to bear an index round it, which will point out the angle of lee way on the compass plate. Since the stream below will keep the vane in the ship's true line of motion, if this rod be furnished with a circular protuberance, nearly filling the tube below the spouting aperture, the sudden current of the water within it will be thereby checked and rendered gradual, as was proposed before; and if this rod be divided into two parts just below the protuberance, and if the termination of the lower part be square, and if the bottom of the protuberance contain a square socket, these parts may be occasionally

* Or otherwise, if the external aperture of the tube itself, or of any other tube, could be placed at right angles to the ship's motion, the water would during that time stand at and shew the external level.---N.

separated to plunge and clear the tube when necessary (see note G). The variation of the needle from a polar direction is another source of error in navigation, but the correction by azimuths and amplitudes affords a perfect remedy for it, and when the quantity of variation is discovered, the dead reckoning may be corrected thereby by well known means; also the variation may be found in a more simple manner at all times of the day, by annexing any of the universal sun-dials to the compass box, in such a manner that the meridian of the dial may be parallel to its side, and by adjusting it to the latitude of the place; for if such a dial be turned until the shadow points to the hour found by a lately corrected watch, its meridian, and the side of the box to which it is attached, must be in a true north and south direction, and the deviation of the north point of the card from this meridian will be the angle of variation. (See note E)

Currents are the sources of the greatest errors to which navigation is liable; the method of finding their set and drift by the course and rate at which a sunken body separates from a drifting boat is erroneous, being grounded on a supposition, that this body meets with still water, whereas it is believed, that such a mass of water moves in a current together, that our experiments only penetrate its surface; however, this method seems to be sufficient to determine the existence of a current, and the rhumb on which it flows, because every stratum of moving water runs with a different degree of velocity, and this will be the case even if the sunken body should descend into still water, or into a contrary current (and since intersecting currents have never been observed, we may conclude, that this experiment will, in all cases, prove the existence of a current, and shew the line of its motion, though it remain uncertain to which extremity of that line it may flow.

If the preceding methods of determining course and distance can be relied on, the error arising from a current which does not flow in a parallel of latitude may be ascertained by attributing the error of dead reckoning to the line of the currents motion, and then by adding or subtracting, as the case may be, such a supplementary distance in the direction of that line, as will make the dead reckoning correspond with the latitude by observation, which will give the true place of the ship; and if the currents be regular and periodical, as is supposed, numerous experiments made in this way at various times and places, might afford materials for the construction of tables, or hydrographic charts, pointing out the drift set and periods of these currents, which would enable the navigator to make the necessary allowance for their effect in all cases.

Also if the true direction of the wind can be ascertained in a current, the lines of its motion being known, the set and drift may also be known, because if the ship lie to, the apparent wind will be a composition of the true wind, and of the wind arising from the relative motion of the air produced by the running water (see note F), which, in most cases, bears a very sensible proportion to the true wind (see *Encyclopædia Britt.* Vol. XVII. p. 204); therefore if the vessel sail along the current line, varying the rate of her motion until the wind vane lie exactly in the direction of the true wind, there will be no current.

wind,

wind, and, consequently, no current, and the vessel being then stationary with regard to the earth, the rate and direction to the stream will be the set and drift of the current. However, since no adequate method is known to determine the true direction of the wind in a current in all cases, this observation would be useless; but that there are some grounds to suspect that within the tropics the apparent deviations from the trade winds arise from the deflecting force of current winds. If this be so, and if the general motion of the trade wind be assumed as the true direction of the wind, this rule will shew the set and drift of the currents in a great part of the ocean.

NOTES.

A. This law has been denied in cases where a diminution of pressure on the back part of the moving body arises from its motion, but the altitude of the water in this tube is affected only by the *absolute*, and not the relative, impulse of the impinging current (which is apparent from the form of it), and Mr. Vince's late experiments tend to prove the truth of the theory in such cases.

B. If the spouting aperture be very small, the theory is true, but when the aperture is large, it is not so certain for this reason; and also because it will be more convenient to receive a small than a large quantity of water into the ship, the hole through which the water issues should be very small, with even thin edges. See Vince's *Hydrost.* p. 54.

C. It may not be useless to observe, that if the space represented by the instrument in passing over any certain number of such waves be observed and divided by the number of these waves, it will give what is called the breadth of the wave; and since the celerity of a wave is demonstrated to be as the square root of its breadth, the rate at which the wave moves may be found by a table at any time by the help of this instrument.

D. Nairne's marine barometer is formed on this plan, and also the tide gauge.

E. It may be curious, if not useful, to observe, that it is possible to steer by the sun alone without the aid of magnetism.

If there be a large watch, whose hand performs one revolution in twenty-four hours, with suitable graduation on the dial plate; this dial plate, with a perpendicular style in the centre, when reclined according to the latitude, becomes a sun dial; and when the shadow of the style coincides with the hand of the watch, the style must hang over the true meridian: therefore if this meridian be set to the ship's course by a motionless compass card, and if the helm be so managed as to keep its shadow constantly on the hand of the watch, the true position of the ship will be preserved until the time of the watch or the elevation of the style require correction. It is possible to effect the same thing by a star, by having a dial plate formed of some reflecting substance.

In climates where the sky is seldom obscured, such an instrument would afford a steady guide to the mariner, not being liable to the errors of variation, aurora borealis, or to the deflections of libration, which disturb the suspended card very considerably.

F. It is a fact, well known at sea, that the apparent wind is composed of the true wind, and of the wind occasioned by the ship's motion; therefore it is that a ship sailing on different tacks, or with a different degree of velocity in the same line, will have the wind in a different point, because one of the component forces is altered in position in the former, and in intensity in the latter case. What we experience in riding through a shower of rain is a familiar example of this fact; while the horse's motion is uniform, the direction of the drops appears to be constant, but if it be encreased, the direction of the drops will be more in the rider's face; if it be diminished, it will be more behind him; and if his road be changed to the right or left, the shower will apparently alter to a point more opposite to his motion than the point it seemed to be in before.

G. Should it be considered adviseable to disencumber this instrument by separating the lee-way machinery from it, it may be annexed to the compass box in the following manner:

A metal rod is to be extended down the stern post, and so hung as to be turned easily on its axis by a vane at the bottom, which is to be acted on by the stream. To the upper part of this rod, just below the level of the deck, a cross bar of a few inches long is to be annexed. The compass box must rest on an upright foot, extending a little below the deck, and supported by a socket, in which it is to turn freely. Just below the deck a cross bar is to be annexed to this foot equal to that on the stern post rod, and the extremities of the two cross bars are to be connected by two slender equal bars moveable on pivots, so that the four bars may form rhomboides, in consequence of which the opposite sides must be always parallel, and the motion of the foot will be the same as that of the stern post rod: therefore the vertical section of the compass box, which passes through the centre and steering mark of the box, will be parallel to the vane; and since the stream will point the vane exactly to the ship's true line of motion, it follows, that the steering mark on the compass box will be pointed to the true line of motion also, and the angle contained between this mark and the north point of the card will consequently be the true magnetic course clear of the error of lee-way.

IV.

On the Escapements of Time Pieces. By Mr. THOMAS REID.

TO MR. NICHOLSON.

S I R,

Edinburgh, April 6, 1801.

BEING lately favoured (by a friend, and one of the most eminent philosophers of this place) with a reading of your useful Journal, chiefly with a view that I might see the account given of Mr. Prior's escapement, in which I was somewhat interested, having made one of the same kind in the year 1793, and which watch is at present in the hands of Mr. Howells, No. 15, White Hart Row, Kennington Lane, Surry.

The originality of invention that seems to attach to Mr. Prior, I lay no claim to; as by a note it appears that he had not seen the escapement of Mr. Mudge, and I may add, nor perhaps that of Lepaute's. Although I had not seen Mr. Mudge's escapement, some hints respecting it were given me in an obscure manner, but yet so as by them I could comprehend it. The escapement of Lepaute's I was perfectly acquainted with; and it was in attempting to combine his with Mudge's that led me to make the one now in question.

In mine, the time of rest of the teeth, and the flanch for the impulse, is on the inside of the pallets; and in that of Mr. Prior's they are on the outside.

There is a very material circumstance which Mr. Prior has not perceived. His pins are arranged in the same line of circle on both sides of the wheel. This will make a considerable difference, especially if the pins should be left pretty thick, which will be the same as if the pallets were impelled by wheels of unequal diameters, and they will consequently receive unequal impulses.

That pallet which moves inwards, receives its impulse from the outward part of the pins. The pins ought to be so disposed as to give equal impulses. For that purpose, let a very fine circle be traced on the side of the wheel, let the holes for the pins, that are to act on the pallet outwards, be drilled inside of the circle, and so as that the edge of the holes shall coincide with it: those pins for the inward pallet must be drilled without the circle, and to have the edge of their holes also to coincide. In this case, the impulse given to the pallets will be equal; but then it may be said that the rest is not equal, this last is so trifling, that of evils it is the lesser.

In this case of an escapement wheel *with pins* on each side, the impulses and times of rest are brought nearer to an equality than in Mudge's, yet his teeth have some advantages which the pins want.

The roller attached to the verge, and serving as a pallet, is not new. Le Roy, Berthoud, and others, had used such, nor do I see much benefit to be derived from it. The watch-makers of Chaux de Fonds, Locle, and of Geneva, think so little of this, that they use simply,

simply a piece of brass for this pallet. You must know that *they* have long ago and often made up watches and clocks with Mudge's escapement to them; probably from Berthoud's carrying it to Paris with him; Mr. Mudge having shewed him the escapement when he was in London, in 1766.

I see you wish to be informed, Who was the inventor of the compensation watch at present in use? I can inform you this, it was the invention of Le Roy, as you may see in a book, entitled, "*Voyage fait par ordre du Roi in 1768, pour éprouver les montres Marines, inventées par M. Le Roy, par M. Cassini, Fils.*"

The invention of the horizontal escapement you have attributed to Debaufre. The escapement of Debaufre has not the smallest affinity to the horizontal one, nor can it give any idea of it, or tend in the least to lead to it.

Mr. Tompion certainly gave the idea of the horizontal escapement, in 1695; to be sure it was not in his hands just what it now is, but in the state such as he left it, was Mr. Graham (who, I believe, was his apprentice) afterwards led to improve it, and ultimately to complete it, prior sometime to 1730.

Debaufre's escapement had two balance wheels on the same axis, and was a contrate wheel watch. It was the first watch that perhaps was ever jewelled in the verge holes. In his attempting to improve the escapement, he formed a connection with a M. Facio, a native of Geneva, who, in 1700, had invented the piercing of rubies and of watch jewellery, and not being well received with his invention at Paris, Facio came over to London; and set to work with Debaufre at the said escapement: its pallet was of diamond.

There are watch-makers at present in London who may remember a Debaufre, a watch-jeweller, and one of the earliest of that business, who died about 1774, or thereabouts: he was old, and perhaps might be the son of the former. The introduction of watch jewellery* into Britain, you see, arose from an accidental circumstance; and it is strange that to this day it is hardly known in France, unless of late some one may have left this country, unfortunately by want or by opinion, and carried the art there. Should these lucubrations be favourably received, I may have something more to communicate to you afterwards.

I am SIR,

With much esteem,

Your most humble servant,

THOMAS REID.

Watch-maker, Edinburgh.

* Watch jewellery has contributed not a little to the celebrity of our watches.

Pray who first applied the duplex escapement, in London, to watches? It is the invention of a Dutertre early in the eighteenth century *. How long is it since watches with the virgule escapement were seen? and who was he who applied it first?

See *Regle Artificielle du Temps*, par M. Sully, respecting Debaufre's escapement; it is but rather treated of there obscurely, yet I am quite well aware of its construction. In the spelling of escapement I have used an ellision for the *e*; however, you may adopt which way you think best. We use the word '*scapement*' in common conversation.

I wonder much at those gentlemen of l'Academie Royale des Sciences so grossly mistaking or misrepresenting what Sully has written; nothing so clear to me as the account Sully gives of Tompion's escapement being the foundation of the horizontal one. Tompion gave it up for a bad job, and this might be one reason why it was so late before it was completed under the hands of Graham. Those gentlemen of the academy are equally mistaken in their ideas of improvement given by P. Le Roy to Debaufre's escapement, which they call the horizontal. What P. Le Roy did was, he took one of the wheels, and substituted another pallet; but nothing here all the while but is most distant from the horizontal one. It is true that Sir Isaac Newton had one of Debaufre's watches, which he showed to Sully in 1704. I do not know who this Gourdain was, who is also mentioned. It must be Gaudron that they mean. He was a man of genius, and watch-maker to the Duke of Orleans, during the Regency †.

* N. B. Enderlin takes the merit of being the inventor as well as Debaufre, and P. Le Roy claims that of Dutertre. See Lepaute, Thiout, &c.

† Gourdain was a watch-maker probably of Paris. His communications were made to the academy in 1742.

The commission of the academy, in their report on M. Gourdain's improvement of Baufré's escapement (*Recueil des Machines*, VII. 137), describe this last as follow: I translate verbally.

"To remedy this inconvenience (the acceleration in the crown wheel and pallets, when the first mover is augmented) "the Sieur Baufré, a French watch-maker, settled in London, imagined, in 1704, the "escapement called dead beat (*qu'on nomme à repos*). He suppressed the two pallets and the crown wheel, "and fixed on the arbor of the balance a kind of cylinder, rather thick, interrupted in one of its halves, "and which, instead of its being terminated in its section by the plane which would pass through its axis, "is on the contrary (terminated) by two inclined planes, one on each side of the axis.

"Opposite these two inclined planes are two wheels fixed parallel to each other on the same axis, placed "so that the teeth of the one are opposite the interval formed by the teeth of the other. By this means, as "soon as one tooth has slid along one of the inclined planes and impelled the balance, the other tooth "reposes on the superior base of the cylinder which carries the inclined plane; so that the balance can obey "the whole impulse it receives, and run through arcs as large as are necessary, which gives a facility of "regulating the going of the watch, and taking advantage of all the accuracy which the spiral spring attached to it is capable of giving."

The reader will perceive how I myself, and not the academicians, have been mistaken in my note at page 53 of the second volume of this Journal, in concluding this to be the horizontal escapement, to which, indeed, the description would apply if we were to overlook the words here put in *Italic*, as I did, and infer that the places of repose were on the curved surfaces of the cylinder.

Gourdain's escapement is precisely enough described in the note last quoted.---N.

What has given credit to Pinchbeck as the inventor of a watch thermometer, must have been the circumstance of his purchasing from Berthoud, when in London, 1766, for his present Majesty, a watch with center seconds, the common Verge Escapement, and having a thermometer, or compensation piece, which Berthoud says, was the first pocket watch made with such a thing. Kendal copied this compensation work in some watches that he made afterwards.

Sully wrote his *Regle Artificielle* in 1717, and J. Le Roy gave an edition of it in 1737. He was an English watch maker; what time he went to France I know not, but he was a great genius, much esteemed there, and had the merit of putting the business in a better train, reared up workmen, and gave them quite new methods. Julien Le Roy thought highly of him, and was himself a man of very uncommon ability.

Excuse this desultory manner, a little trouble may make it more connected.

T. R.

V.

Inquiries relative to the Laws of Affinity; by CIT. BERTHOLLET, Member of the National Institute of Arts and Sciences.*

(Concluded from page 21.)

ARTICLE X.

On the Determination of the Elective Affinities.

1. **T**O determine the elective affinity of two substances for a third, according to the notion which we ought to conceive of it, consists in ascertaining in what proportion the third substance divides its action between the two first, and to what degree of saturation each of these must arrive when their forces are equally opposed. The relative affinity will be in proportion to the degree of saturation they would attain, on account of the quantity which shall have acted; so that if the quantities were equal, the comparative degrees of saturation would afford the measure of the respective affinities.

2. When I speak of the saturation of a substance, I do not mean that absolute saturation in which all reciprocal action would cease, but a degree of saturation which is easily distinguished, and is common to all combinations. It is that of neutralisation, in which none of the properties of the constituent parts predominate. The term of crystallisation of salts, does not always coincide with that of neutralisation; for example, with respect to the carbonates of alkali, which still continue to afford indications of alkalinity, and with the acidulous tartrate of pot-ash, which on the contrary retains an excess of acid. Nevertheless, we may take the last combination at the term in which it is neuter, because it still possesses

* Translated from the *Annales de Chimie*, XXXVII, 151.

the property of crystallising; it is this tartrate also which is necessarily obtained, when an excess of the base is present in the experiment; but when an excess of acid is required, the degree of saturation in which the acidulous tartrate exists, may be determined by the quantity of pot-ash which is necessary to effect its neutralisation.

3. One consideration which is immediately seen to deserve much attention, is, that on comparing the affinities, it is necessary to employ in all the experiments, the same proportions of all the substances which might be successively put in opposition; because, if the proportions vary, the result of the action will be no longer the same, nor can the affinity be represented by the same number. I shall proceed to illustrate this observation by an example:

Let the number 100 represent the pot-ash, which is to be saturated by 100 parts of sulphuric acid; I oppose to it 100 parts of soda; I suppose that after the action the pot-ash will be found to have absorbed 60 parts of the acid, and the soda 40; I shall then conclude, that the affinities of those two bases for the sulphuric acid are in the proportion of 60 to 40. But 40 parts of pot-ash remain uncombined; these still continue to act, and contribute by their action to the division of the acid; so that if this quantity were to be varied, the result would no longer be the same. If instead of 100 parts of pot-ash and 100 parts of soda, I take only 80 parts of each, the uncombined remainder will afford 20 parts of pot-ash, and another quantity of soda, so that the forces exerted by those two parts, are no longer in the above-mentioned proportion; whence it appears that the two saturations cannot be in the proportion of 60 to 40.

4. But in order to ascertain the degree of saturation acquired by each of these substances, it is necessary to effect a separation; and this can only be obtained by elasticity, by crystallisation, by precipitation, or by the action of a solvent. But we have seen that these different means ought to be considered as foreign powers which alter the results, and determine the combinations that are formed, without our being able to measure their effect, so as to disengage that of the elective affinity; and consequently, that those separations which, in the articles I, II, III, have only been considered as the proportional effect of elective affinity, are really the effect of several joint forces, as is proved by the observations subsequent to those articles.

When it is necessary, for instance, to ascertain the quantity of sulphate of pot-ash and soda that might be formed, the force of cohesion not differing considerably in these two sulphates, it is probable that it would not effect any great change in the proportion of the two salts which would crystallise; but it would be necessary to separate the excess of alkali by alcohol, in order to obtain the whole crystallisation. Now, the action of the alcohol being unequal upon pot-ash and soda, would again produce a change. To these considerations we may add, that a change of proportions would not only cause the force of the soda and the pot-ash (No. 1) to vary, but also that of the alcohol, without mentioning the affinity of the water, which serves as the solvent.

But the changes would be much more considerable, if barytes were to serve as a comparison with either pot-ash or soda; the force of cohesion possessed by the sulphate of barytes would then operate in such a manner, that it would leave the alkali only a very small quantity of sulphuric acid, which would be much nearer to a measure of the force of cohesion of the sulphate of barytes, compared with that of the solvent, than of the affinity of the barytes compared with that of the alkali.

This is so true, that if, in order to compare the affinity of the acids for barytes, we were to begin by treating barytes with an excess of sulphuric acid, it would be almost entirely precipitated, unless the acid were extremely concentrated; we could not distinguish the combined portion from that which is not, and assert, that barytes has a greater affinity for the former part than for the latter. This however is really stated, when it is affirmed, that the sulphuric acid has a greater affinity for barytes than any other acid, because a sulphate of barytes is formed by precipitation. That effect is attributed to elective affinity, which depends more particularly upon the force of cohesion peculiar to the sulphate of barytes.

It is therefore manifest that the elective affinity of any two substances, with regard to a third, cannot be determined by a direct experiment, even when the trial is made with two substances in a liquid state, and which can be neutralised by saturation; since it is necessary to introduce foreign powers in order to ascertain the saturation.

5. It has been seen in articles II and III, that the affinity of a substance may be compensated for by its quantity.

From this consideration, it would appear to be sufficient if we were to discover the capacities of saturation which different bases have for an acid, or different acids for a base, in order to ascertain the proportion of their relative affinity: for this should seem to be in the inverse proportion of the quantities necessary for producing the same degree of saturation.

This consequence is erroneous, when we wish to apply it to the elective affinities, because as soon as two substances are presented in order to combine with a third, new forces are established, which not only determine other results, but even change the constitution of those substances.

I compare the sulphuric and carbonic acids. It is certain that if I bring a quantity of pot-ash to a term of neutralisation by the carbonic acid, it exerts a force as great as the quantity of sulphuric acid which would be necessary to produce the same effect; and yet if I pour the sulphuric acid upon the first mentioned combination, the whole of the carbonic acid will be disengaged; because, being no longer retained by an equal force, it again assumes the elastic state; and even if it be absorbed and retained by a sufficient quantity of water, it will no longer be in the same state of compression; it will not have the same constitution; it will no longer be the same substance relative to chemical action. And lastly, we must apply the observations made in the preceding numbers to the action of substances unequally saturated.

The comparison between the capacities of saturation, though it may lead to important considerations, cannot therefore be applied to the determination of elective affinities.

ARTICLE

ARTICLE XI.

On some Errors which proceed from a false Notion of Elective Affinity.

1. I shall here discuss several opinions which have been adopted relative to elective affinity; I shall shew how unfounded they are, and shall oppose to them the application of the principles laid down in the preceding articles:—

Baumé has observed, that when the sulphate of pot-ash is dissolved by means of heat, in an equal weight of nitric acid, crystals of the nitrate of pot-ash are obtained by cooling: he attributes this decomposition of the sulphate of pot-ash to reciprocal affinities which produce opposite combinations, but does not determine the cause of this contrary effect.

2. The explanation of this remarkable fact has been contradicted by Bergman. He observes, that there are some salts which have a tendency for an excess of acid, such for instance as the acidulous tartrate of pot-ash. He conceives that when these salts exist in a state of neutralisation, it is necessary to consider their base as divided into two parts; one of which is particularly affected by the whole action of the acid, in order to form the acidulous salt, while the other part only tends to satisfy that portion of acidity which remains in the acidulous salt. This part of the base is therefore only retained by a feeble acidity, and may be carried off by an acid very inferior to that which enters into the first combination. Thus the acetous acid may carry off that part of the pot-ash which, in the tartrate of pot-ash, is superfluous to the combination which constitutes the acidulous tartrate of pot-ash, though this acid has an affinity much weaker than that of the tartareous acid itself.

Now, the sulphate of pot-ash is among the number of the salts which have a tendency to form an acidulous salt. Nearly two thirds of its base enter into this combination, and it is this part alone that is subject to the whole affinity of the sulphuric acid. The remaining third may be separated by an acid of an affinity inferior to that of the sulphuric acid, such as the nitric acid, the muriatic acid, and the tartareous acid. When the decomposition is carried to this point, it ceases, whatever may be the quantity of the acid opposed: and if the quantity be not too great to prevent the crystallisation, or if the excess be expelled by heat, an acidulous sulphate is obtained by solution and evaporation, which forms permanent crystals in the air.

3. How could the illustrious Bergman have deviated from the path traced out for him by observation? It is proved even by his own experiments, that the acid which is superabundant to the acidulous sulphate of pot-ash exerts its affinity, that it exists in combination, and acts in proportion to its quantity. For he observes, that if sulphuric acid be added to the acidulous sulphate of pot-ash, this salt is dissolved, and loses its property of crystallising; that this excess of acid is expelled with difficulty, even by distillation in a retort; and in order to obtain this effect, it is necessary to fuse the saline combination in a crucible, or to employ several times the repeated action of very pure alcohol.

4. The limit given by Bergman to the action of the acids upon the acidulous sulphate is therefore imaginary. This sulphate acts like all the salts which are capable of resisting, to
a certain

a certain degree, the action of an excess of acid or of base (Art. V. No. 4), as well as that of another acid, or a foreign base. The only difference between them in this respect depends on the force of cohesion, which may act more or less strongly to produce crystallisation, which is peculiar to certain proportions of the acid and its base, probably in consequence of the figure assumed by the particles of which the combination consists.

5. When an acid has the property of forming a precipitate, by combining with a base, it has been supposed to possess a greater affinity for that base than the acid with which it was before combined, without even examining to what point the new acid may have effected the decomposition; and without considering that an opposite decomposition would have taken place merely by a change of the proportions, and might consequently in that case have justified an opposite conclusion.

In this manner as the tartareous acid has the property of forming, with pot-ash, an acidulous salt of difficult solubility, and consequently affords a precipitate with all those salts that have pot-ash for their base, and are not diluted with too great a quantity of water, it has been supposed to have a greater affinity for pot-ash than the other acids. Bergman has made an exception with respect to the sulphuric acid, because he supposed that the tartareous acid could only act upon that pot-ash which exceeds the combination of the acidulous sulphate of pot-ash; a supposition which I trust has been refuted in the preceding numbers. He has also excepted the nitric and muriatic acids, because he imagines that the tartareous acid acts, with respect to the nitrate and muriate of pot-ash, in the same manner as with the sulphate; and nevertheless he has not ascertained the existence of an acidulous nitrate and muriate of pot-ash similar to the acidulous sulphate of pot-ash.

He also concludes, from experiments made with the salts which have soda for their base, without however making known these experiments, that the tartareous acid ought to be placed after the oxalic acid; but, these exceptions apart, the tartareous acid, according to him, completely decomposes all the other salts that have a fixed alkali for their base.

What embarrasses Bergman is, that the tartareous acid does not produce a precipitate with the salts which have soda for their base. He believes that this apparent difference is a consequence of the soda not having the property of forming a salt of sparing solubility with an excess of acid; but then the indication which is thought to afford evidence of the decomposition is wanting; and it becomes necessary to be satisfied with the probability, that the affinities of one fixed alkali follow the same order as those of the other.

All this classification of affinities is founded upon the false supposition, that one acid will expel another from its combinations by its affinity alone, considered as a constant force; and this supposition necessarily demands several others, in order to explain as exceptions, those facts which are naturally derived from a general property.

6. I have examined the decomposition of the acidulous tartrate of pot-ash, by the nitric acid, which, according to the received opinions, then also adopted by me, ought to decompose it, by completely depriving it of its base. I therefore put some acidulous tartrate of pot-ash and nitric acid in digestion together, and obtained by cooling, some fine crystals of nitrate

of pot-ash. I repeated the experiment several times, by adding nitric acid till nitrate of pot-ash was no longer separated. I then exposed the liquid to heat capable of making the disengaged portion of nitric acid evaporate, without altering the tartareous acid. After this the liquid had acquired an oily consistence; it had no smell, and did not announce the existence either of the nitric acid or of pot-ash; but on exposing it to a strong heat, much nitrous gas was disengaged, the tartareous acid was reduced to coal, and its ashes afforded a considerable quantity of carbonate of pot-ash.

7. In this operation one portion of nitrate of pot-ash is separated by the force of crystallisation in that salt, as far as that point at which that force is overcome by the superabundant acid. The acidulous tartar is rendered soluble by the action of the nitric acid, which, at the same time, deprives it, by crystallisation, of a part of the base necessary to make it insoluble.

On the other hand, the tartareous acid, poured on a solution of the nitrate of pot-ash, deprives it of its pot-ash to a certain degree, and forms an acidulous tartarite which is precipitated; but as it has not the property of forming an acidulous tartarite of soda, of sparing solubility, it does not produce any precipitate with those salts that have soda for their base.

In both circumstances, all that portion which cannot be separated by the force of cohesion, forms a liquid, in which the substances act in proportion to their present masses.

Nothing, therefore, can be concluded about the respective affinity from such separations as are effected by precipitation or crystallisation, since by the mere change of proportions, opposite decompositions may frequently be obtained.

8. The precipitations which take place when, on comparing the affinities of bases, one of them forms an insoluble combination, has given rise to an error of the same kind; and on this ground alone it has been established that lime has a greater affinity than alkali for the fluoric, phosphoric, and arsenical acids, or, in a word, with all those which form with it an insoluble compound; and that consequently it has the property of entirely decomposing salts formed by the alkali and these acids. This precipitation is not the result of elective affinity, and it is not complete; but its quantity is determined by the proportion which the action of the fluid bears to the force of cohesion in the precipitate: hence it is that the precipitate is frequently dissolved again on increasing the quantity of the liquid substance opposed to it.

9. Although Bergman has well described the changes which heat is capable of producing in chemical action, when the substances have a tendency to become volatile, and has also warned us against employing too strong a heat in evaporation, the whole influence of heat in those operations by which salts are separated, in order to form a judgment of their affinities, has not yet been shewn.

It would not have been concluded that the sulphuric acid has more affinity for the fixed alkalis than the nitric or muriatic acids, from the simple circumstance that, by a strong heat,

heat, it expels those acids from their combinations; if attention had at the same time been paid to the fact, that even by the heat employed for causing the evaporation and obtaining the crystallisation of salts, the proportions of the volatile acids may be considerably changed by the sulphuric acid which remains opposed to them, and which may even at last expel them entirely, by means of the difference which exists between its fixity and that of those acids. (Art. VII. No. 5).

10. We are indebted to Bergman for some useful observations relative to the errors that may originate from the solubility of a substance which is disengaged, and of which the separation is not perceived. He remarks that pot-ash or soda do not affect the transparency of a solution of salt with bases of lime, if this solution be diluted with fifty times its quantity of water; because the lime which is separated, being soluble, remains in the water. But he does not consider that if the lime did then possess only its natural solubility, this would be a very feeble obstacle to the precipitation; for it would require nearly seven hundred parts of water to effect its solution. What adds so greatly to its natural solubility is, that it continues to remain in combination with the acid (Art. V. No. 5), and that it cannot be separated without retaining a part of this acid (Art. III. No. 9), by which its solubility is so increased.

11. Notwithstanding his general observations, Bergman has mistaken the effects of solubility in a variety of circumstances. For instance, he did not believe that the nitric and muriatic acids were capable of acting upon the phosphate of lime, though the only difference that can be ascertained in this respect between these acids and the sulphuric, without being acquainted with the comparative force of their affinities, is, that the two first-mentioned only form soluble combinations, while that produced by the sulphuric acid may in a great measure be withdrawn by the force of crystallisation.

12. It is the solubility of lime, as well as of barites, acquired by the action of the acid, upon those earths, which prevents ammonia from affording any precipitate in the solution, of those salts of which they are the base. Nevertheless the first portion of ammonia which is mixed, for instance, with the muriate of lime, emits scarcely any smell, which indicates that it has entered into combination; and its action may be rendered perceptible, as we shall shew.

I mixed some ammonia with a solution of muriate of lime, and evaporated the liquid in a retort. When it was reduced to a certain point, a considerable quantity of precipitate was formed. I continued the experiment: towards the end the quantity of precipitate was much diminished: a pellicle was formed, and by cooling a great quantity of crystals in needles of a tolerable length were obtained. It was a triple salt, from which ammonia might be disengaged by lime. This salt, on being again dissolved and evaporated in the open air, no longer afforded indications of ammonia when tried by lime.

It is therefore evident, that when the water was not too abundant, the ammonia precipitated part of the lime, though it was rendered much more soluble by the acid, and though the action of the ammonia was considerably weakened by heat, which diminished both

both

both its affinity and its quantity. In proportion as the latter was reduced, the precipitate was again dissolved; notwithstanding which, there were some remains of ammonia after a long evaporation, and it was only by means of the action of the air that it was entirely dissipated. The separation of the lime would doubtless become much more sensible, if ammoniacal gas were received in a slightly diluted solution of the muriate of lime.

When ammonia produces a precipitate with the salts that have alumine for their base, it is because this earth is less soluble than lime, even when it is combined with the portion of acid which it retains when precipitated.

ARTICLE XII.

On Complex Affinities.

1. I shall proceed to examine, under the more general name of *complex affinity*, what has been considered as due to the concurrence of four affinities, and which has been commonly known by the name of *double affinity*.

To give an idea of the action of four affinities, Bergman examined the effect which takes place on mixing the solution of the sulphate of pot-ash with that of the muriate of lime. It is, says he, the same as if the proportions of sulphuric and muriatic acid, of lime and pot-ash, which entered into the composition of these salts, were put in the quantity of water employed; the two bases act by their affinities upon the two acids: but though the affinity of the pot-ash for the sulphuric acid be stronger than what it has for the muriatic acid, yet the affinity of the latter for the pot-ash, added to the affinity of the sulphuric acid for the lime, gives an aggregate of forces greater than the affinity of the sulphuric acid for the pot-ash, and that of the muriatic acid for lime: thus an exchange of bases is determined; so that instead of sulphate of pot-ash and muriate of lime, we have sulphate of lime and muriate of pot-ash. This explanation is always founded on the supposition that the affinities are constant powers, independently of the quantities and state of saturation.

2. When two bases act together upon an acid, the latter is divided, or rather divides its action in proportion to their masses; and if instead of one we have two acids, and no separation is effected either by precipitation or crystallisation, both the acids will act equally upon the two bases in the proportion of their masses. If each of these acids were originally combined with a base, the amount of the mutual forces of the acids and alkalis, after mixing the solutions of the salts, will be the same as before. No muriate of pot-ash nor sulphate of lime are formed; but there will be a combination of pot-ash, lime, sulphuric acid, and muriatic acid, which will give the same degree of saturation as before the mixture. Hence it is, that on mixing two salts, which, by exchange, ought to produce combinations that would have very different proportions of principles, there is not, as has been justly remarked by Guyton, either the acidity or the alkalinity which would necessarily appear if such a change were really to take place.

3. The exchange of bases has been inferred merely from the result of the precipitation and crystallisation which have been observed; but this effect has not been attributed to its real cause.

In Art. V. it has been shewn that the force of cohesion determines the separation which, in the elective affinities, takes place by precipitation or crystallisation. It is also the same force which produces the same effect in the complex affinities. When I mix the solution of sulphate of pot-ash with that of the muriate of lime, the quantity of water not being great, the lime, in its contact with the sulphuric acid (No. 1), is subject to the effect of the force of cohesion in a higher degree, than the pot-ash. Thus a new force is added to those which previously existed; and this must determine the combination of the sulphuric acid with the lime, at the same time as its precipitation.

4. If we take a rapid view of all the known decompositions which are owing to the complex affinities, and we shall see that it is always to the substances which have the property of forming a precipitate, or a salt that may be separated by crystallisation, that an excess of affinity has been attributed beyond those opposed to them: so that we may foretel by the degree of solubility of the salts which are formed in a liquid, which are the substances of which Bergman and other learned chemists have pretended to represent the forces in their symbolical tables, by always attributing a superiority of affinity to the two substances which are disposed to form an insoluble combination relative to the quantity of the solvent.

Lime, magnesia, barites, and strontian, form, with the carbonic acid, insoluble salts; all the soluble combinations of these earths, when mixed with carbonates of alkalis, produce an exchange, from which the formation and precipitation of carbonates with earthy bases result.

Barites forms, with the sulphuric acid, an insoluble salt. Every time the solution of a sulphate is mixed with that of a salt with base of barites, a salt having a similar base, is formed and precipitated.

As lime makes a sparingly soluble sulphate, which is in a great measure precipitated, if there be not much water, it also changes its base with all the soluble sulphates, till the period when precipitation ceases, by the solubility of the sulphate of lime. The sulphate of lime being still more soluble than the sulphate of barites, the more soluble salts with base of barites decompose the sulphate of lime.

The oxide of silver forms an insoluble salt with the muriatic acid: all the salts of silver which are soluble, being mixed with soluble muriates, the muriate of silver is precipitated. Mercury, which is not too much oxidized, acts in the same manner.

As the muriate of lead is scarcely soluble, the salts which the oxide of lead forms with the other acids, and which possess solubility, produce a precipitate with the soluble muriates; but as it makes an insoluble salt with the sulphuric acid, the solution of muriate of lead affords a precipitate of sulphate of lead, when mixed with soluble sulphates.

5. When

5. When, therefore, water is evaporated, in which several salts have been put in a state of solution, they will be found to separate according to the order of their solubility, and it is from this property that we are able to ascertain the changes of base which may be effected.

But the solubility of salts varies according to the temperatures. It is, therefore, the solubility relative to the different temperatures which ought to be considered. The nitrate of pot-ash, mixed with the muriate of soda, will crystallise at a low temperature; but the muriate of soda will separate even during the time of evaporation. No exchange of bases will be effected, because the nitrate of soda is a little more soluble in cold than the nitrate of pot-ash, and on the contrary, the muriate of pot-ash is rather more soluble in heat than the muriate of soda.

6. I here attend only to the principal result, arising from a force of cohesion so great, that it causes the effect of the forces opposed to it to disappear. But when this power is not considerable in the combinations that may be formed, the mutual action of the substances which remain in the liquid state, that of the solvent, and the proportions which vary by the crystallisation of a combination that is separated, must produce different effects. The experiments I have begun will not fail to elucidate this subject.

7. Another circumstance may change the action of the complex affinities; namely, the formation of a triple salt which precipitates; but by knowing the degree of solubility of this combination, we may still foretel the decomposition that will take place. The same consideration is applicable to the affinities called *elective*.

8. A precipitate is sometimes effected by the mixture of two saline substances which have the same acid; for instance, by a mixture of the muriate of magnesia and the muriate of lime. It is probable that two combinations are then formed, one with an excess of acid and a small part of the two bases, the other with the greatest part of the two bases and a small portion of acid.

This effect is analogous to what we have observed in Art. IX. No. 3; but here it is the mutual affinity of the two bases which determines the precipitation.

9. We have seen in Art. VII. that heat, by increasing the volatility of a substance, weakens its combination. This cause acts no less in the complex than in the elective affinities. It is a force added to those which act, and determines the union and the separation of those substances which have the greatest tendency to form a volatile combination.

Whenever, therefore, it is desired to know what will happen, by exposing two salts to the action of heat, it is only necessary to examine whether one of the two bases and one of the two acids possess a greater degree of volatility than the remaining base and acid, and we may be assured that, on applying a sufficient degree of heat, the combination of the most volatile base and acid will be formed and sublimed, while the more fixed base and acid will also remain combined. Among the bases, ammonia and the oxide of mercury, and among the acids, the carbonic and muriatic, afford several illustrations of this truth.

10. Efflorescence ought also to be considered as a force which, in the complex affinities, may determine a combination possessing this property, and to this is owing the formation of natron in the valley of the Lake of Natron, and other places where the same circumstances occur.

The observations which I have presented to the Institute of Egypt, which will form a sequel to the interesting discovery of the valley of the Lakes of Natron, for which we are indebted to General Andreossi, prove that the circumstances necessary to the formation of natron are, 1. a sand, which contains much carbonate of lime; 2. moisture; and, 3. the presence of the muriate of soda: I have also remarked that the stalks of the reed in a great degree facilitate the production of this substance. I have promised to explain the formation of the carbonate of soda by means of these circumstances, which I shall now endeavour to perform.

Calcareous sand, constantly impregnated with moisture, may be considered as a solution of the muriate of soda, which acts upon the carbonate of lime. Now it follows, from what has been explained in Art. IV. that insolubility causes a great diminution in the mutual action of a solid and a liquid substance, but that it does not destroy it. This action is opposed to the insolubility of the carbonate of lime, which is not absolute. A solution must therefore be formed of a small quantity of carbonate of lime, and consequently (No. 1 and 2), the constituent parts of this carbonate and of the muriate of soda, which are in solution, exert a reciprocal action; otherwise the presence of the carbonate of lime would not be a condition necessary to the formation of the carbonate of soda*.

We must, therefore, consider the humidity of the calcareous sand, in which the carbonate of soda is formed, as a solution of muriate of soda and a small quantity of carbonate of lime: hence the soda is found in the presence of the carbonic acid, and the efflorescence, which is a property of the carbonate of soda, ought to be considered as a new force that tends to remove it from this combination. In short, when, in a soil impregnated with muriate of soda, we meet with the stalks of reeds which favour the efflorescence, the carbonate of soda not only accumulates round these stalks, but sometimes it is not formed

* The solution of the carbonate of lime by the muriate of pot-ash, and by the sulphate of pot-ash, which indubitably act like the muriate of soda, has been proved by a direct experiment, for which we are indebted to Guyton (*Mém. de Scheele*, part II. note de la page 18). "The solution of sulphate of pot-ash, muriate of pot-ash, &c. poured in lime water which has been rendered milky by water impregnated with carbonic acid gas, immediately caused the precipitate to disappear. There was likewise no earthy precipitate when water, charged with carbonic acid gas, was poured in a mixture of lime water and a solution of these neutral salts; the liquor always contained a portion of uncombined alkali." Guyton combats the opinion of Scheele, who did not observe any decomposition with the muriate and sulphate with base of pot-ash, but only with the salts with base of soda.

The difference of opinion between those celebrated chemists arose from the circumstance that one ascertained the decomposition only by the efflorescence, which is peculiar to salts with base of soda (Art. VIII.) while the other observed it in a liquid; but in the latter instance it is much more limited.---B.

without such assistance, when certain circumstances, such as a too argillaceous nature of the soil, &c. are little favourable to its production; so that at a short depth muriate of soda only is discoverable.

I wished to ascertain the truth of this explanation in a trough placed in one of the gardens belonging to the Institute. For this purpose some carbonate of lime and siliceous earth, both well washed, were mixed together, to which was added a certain proportion of muriate of soda: a hole was made in this mixture, for the purpose of pouring in water occasionally, and keeping up the necessary degree of moisture. An incrustation of muriate of soda is formed at the surface, which already strongly changes the colour of paper tinged with fernambuc, like the alkalis; but we cannot expect to obtain an efflorescence so considerable as to be perceptible to the sight, till after a much greater length of time.

11. The preceding observations shew that the only difference which distinguishes the complex affinities from those called *elective*, is, that in the former, substances are brought into action which are nearly in an uniform degree of saturation; and that in the latter, there is a substance present which is not yet saturated (or several such substances may be present); so that in the former, a new degree of saturation is established only in proportion to the combinations which are capable of being separated; whereas, in the latter, the action of the unsaturated substances becomes in equilibrio with that of the substances which were already so; whence it happens that the force of cohesion and that of elasticity produce their effect more completely in the complex than in the elective affinities.

VI.

*Experiments on the Solar and on the Terrestrial Rays that occasion Heat; with a comparative View of the Laws to which Light and Heat, or rather the Rays which occasion them are subject, in order to determine whether they are the same or different. By WM. HERSCHELL, LL. D. F. R. S. **

IN the former part of this paper, of which an abridgment was given at page 360 of our last volume, it was shewn that heat derived immediately from the sun, or from ignited terrestrial substances, is occasioned by rays emanating from them, and that such rays are subject to the laws of reflection and of refraction. The similarity between light and heat was the most remarkable feature of the experiments there related. In the present memoir the author proceeds to point out some striking and substantial differences in the rays of light and heat which afford data for discussing whether these two effects be produced by one and the same power.

* Abridged from the *Philosophical Transactions*, 1800, page 437.

The various refrangibility of the rays of heat from those of light was shewn by those experiments with the prism, which undoubtedly constitutes a great point of resemblance. But he points out a very material difference, consisting in the much greater range over which the rays of heat are dispersed when compared with those of light. He has exhibited this by delineating a spectrum of light of a certain assumed length represented by the line G Q (in Fig. 5; Plate III.), divided into seven parts, according to the dimensions assigned to the colours by Newton; and to this he has drawn perpendicular lines, representing the intensity of illumination possessed by the light at each particular part. The length of the maximum perpendicular being arbitrary, he has assumed it to be $\frac{27}{34}$ of the whole line, namely, L R at the confines of the yellow and green. The lengths of the other ordinates to the curve, namely, K M H I N O P, are also determined in relative length and position from his former experiments.

To find a figure in the same manner to express the heating power of the refracted prismatic rays, which may be called the spectrum of heat, he examined the extent of the invisible rays, and found that its line was in proportion to that of the visible spectrum as 2 to 3, or rather, as some allowance must be made beyond the last ordinate, as $2\frac{1}{4}$ to 3. Whence the whole calorific spectrum proves to be as $5\frac{1}{4}$ to 3, with regard to the luminous spectrum. And if the maximum of heat be expressed by a line of the same length as that which denotes the maximum of light, and other ordinates be drawn to express the proportions of degrees indicated by the thermometer at the various points of the whole spectrum, another curve B C D E F G H I K L M N O P will be obtained to express the intensities of heat. From the mere inspection of these two figures, thus drawn as lying upon one another, it is seen how very differently the prism disperses the heat making rays and those which occasion illumination. These rays neither agree in their mean refrangibility nor in the situation of their maxima. At R, where there is most light, there is but little heat, and at S, where there is most heat, there is no light at all!

Experiment 21. The sines of refraction of the heat making rays are in a constant ratio to the sines of incidence. A prism was placed half an inch beyond the last visible red colour of the solar spectrum from a prism, and another at the distance of an inch, while a third was placed beside the spectrum as a standard. The two first exhibited change of temperature, as in the former trials of this kind, while the last underwent no change. Other experiments, with the like event, were tried with prisms of different refracting angles and of different kinds of glass, and also of water. Hence, as the visible part of the rays has long been determined to have the constant ratio here mentioned, and the invisible part always bore the same relative position to these, it must follow that that part also is deflected according to the same law.

Experiment 22. The different refrangibility of heat is corrected by contrary refraction in different mediums. A prism of crown glass, having an angle of 25 degrees, another of flint glass, with an angle of 24, and a third of crown glass, with an angle of 10 degrees, being

being put together as they are placed when experiments of achromatic refractions are to be made, were found to give a spectrum nearly without colour. The thermometer being placed in the middle of this spectrum, had its temperature raised two degrees; but a thermometer placed near either end of the spectrum was not affected. This result shews that the different refrangibility of heat, as well as that of light, admits of prismatic correction, and it also confirms the constant ratio of the sines of refraction to those of incidence.

Experiment 23. In burning glasses the focus of the rays of heat is different from the focus of the rays of light. A burning lens was placed in the rays of the sun, its aperture being reduced to three inches, in order to lessen aberration. The place of the luminous focus was ascertained by throwing hair powder into the air with a puff. A stick of sealing wax was then held 1", 6 or 4 beats of a chronometer in the contracted pencil half an inch nearer to the lens than the focus. In this time no impression was made upon the wax. It was then applied half an inch farther from the lens than that focus, and in eight-tenths of a second or two beats of the same chronometer it was considerably scorched. When the sealing wax was also exposed to the focus of light, it was equally affected in the same time. Whence the Doctor apprehends we may safely conclude, notwithstanding the little accuracy to be expected from so coarse an experiment, that the focus of heat was certainly farther from the lens than the focus of light, and probably not less than one quarter of an inch; the heat at half an inch beyond the focus of light being still equal to that in the focus itself.

In the next place our author proceeds to the subject of the transmission of heat through diaphanous bodies. The experiments made by the prism, the lens, and the mirror, are subject to certain natural imperfections, which must impair the progress of scientific induction. The prism refracts, reflects, transmits, and scatters them at the same time, and the laws by which it acts in every one of these operations require to be investigated. A lens not only partakes of the errors of the prism, but also is subject to the aberration from its spherical figure. And a mirror, besides its natural incapacity of separating the rays of light from those of heat, scatters them very profusely; but our author adds that the scanty provision of materials to act upon these rays has been partly our own fault, as every diaphanous body may become a new tool in the hands of a diligent observer.

(To be continued.)

VII.

*Outlines of Experiments and Inquiries respecting Sound and Light. By THOMAS YOUNG
M. D. F. R. S.**

IT has long been my intention to lay before the Royal Society a few observations on the subject of sound; and I have endeavoured to collect as much information, and to make as many experiments, connected with this inquiry, as circumstances enabled me to do; but, the further I have proceeded, the more widely the prospect of what lay before me has been extended; and, as I find that the investigation, in all its magnitude, will occupy the leisure hours of some years, or perhaps of a life, I am determined, in the mean time, lest any unforeseen circumstances should prevent my continuing the pursuit, to submit to the Society some conclusions which I have already formed from the results of various experiments. Their subjects are, I. The measurement of the quantity of air discharged through an aperture. II. The determination of the direction and velocity of a stream of air proceeding from an orifice. III. Ocular evidence of the nature of sound. IV. The velocity of sound. V. Sonorous cavities. VI. The degree of divergence of sound. VII. The decay of sound. VIII. The harmonic sounds of pipes. IX. The vibrations of different elastic fluids. X. The analogy between light and sound. XI. The coalescence of musical sounds. XII. The frequency of vibrations constituting a given note. XIII. The vibrations of chords. XIV. The vibrations of rods and plates. XV. The human voice. XVI. The temperament of musical intervals.

I. Of the Quantity of Air discharged through an Aperture.

A piece of bladder was tied over the end of the tube of a large glass funnel, and punctured with a hot needle. The funnel was inverted in a vessel of water; and a gage, with a graduated glass tube, was so placed as to measure the pressure occasioned by the different levels of the surfaces of the water. As the air escaped through the puncture, it was supplied by a phial of known dimensions, at equal intervals of time; and, according to the frequency of this supply, the average height of the gage was such as is expressed in the first Table. It appears, that the quantity of air discharged by a given aperture, was nearly in the subduplicate ratio of the pressure; and that the ratio of the expenditures by different apertures, with the same pressure, lay between the ratio of their diameters and that of their areas. The second, third, and fourth Tables show the result of similar experiments, made with some variations in the apparatus. It may be inferred, from comparing the experiments on a tube with those on a simple perforation, that the expenditure is increased, as in water, by the application of a short pipe.

* *Philos. Transactions*, 1800, p. 105.

Table I.

A	B	C
.00018	.25	3.9
.00018	.58	11.7
.00018	1.	15.6
.001	.045	7.8
.001	.2	15.6
.001	.7	31.2
.004	.35	46.8

A is the area, in square inches, of an aperture nearly circular. B, the pressure in inches. C, the number of cubic inches discharged in one minute.

All numbers throughout this paper, where the contrary is not expressed, are to be understood of inches, linear, square, or cubic.

Table II.

A	B	C
.07	1.	2000.
.07	2.	2900.

A is the area of the section of a tube about two inches long. B, the pressure. C, the quantity of air discharged in a minute, by estimation.

Table III.

A	B	C	D
.0064	1.15	.2	46.8
.0064	10.	.45	46.8
.0064	13.5	.35	31.2
.0064	13.5	.7	46.8

A is the area of the section of a tube. B, its length. C, the pressure. D, the discharge in a minute.

Table IV.

A	B	C
.003	.28	46.8

A is the area of an oval aperture, formed by flattening a glass tube at the end: its diameters were .025 and .152. B, the pressure. C, the discharge.

II. Of the Direction and Velocity of a Stream of Air.

An apparatus was contrived for measuring, by means of a water-gage communicating with a reservoir of air, the pressure by which a current was forced from the reservoir through a cylindrical tube; and the gage was so sensible, that, a regular blast being supplied from the lungs, it shewed the slight variation produced by every pulsation of the heart. The current of air issuing from the tube was directed downwards, upon a white plate, on which a scale of equal parts was engraved, and which was thinly covered with a coloured liquid; the breadth of the surface of the plate laid bare, was observed at different distances from the tube, and with different degrees of pressure, care being taken that the liquid should be so shallow as to yield to the slightest impression of air. The results are collected

in Tables v. and vi. and are exhibited to the eye in Plate IV. Figs. 1—12. In order to measure with greater certainty and precision, the velocity of every part of the current, a second cavity, furnished with a gage, was provided, and pieces perforated with apertures of different sizes were adapted to its orifice: the axis of the current was directed as accurately as possible to the centres of these apertures, and the result of the experiments, with various pressures and distances, are inserted in Tables vii. viii. and ix. The velocity of a stream being, both according to the commonly received opinion and to the experiments already related, nearly in the subduplicate ratio of the pressure occasioning it, it was inferred, that an equal pressure would be required to stop its progress, and that the velocity of the current, where it struck against the aperture, must be in the subduplicate ratio of the pressure marked by the gage. The ordinates of the curves in Figs. 13—23, were therefore taken reciprocally in the subduplicate ratio of the pressure marked by the second gage to that indicated by the first, at the various distances represented by the abscissas. Each figure represents a different degree of pressure in the first cavity. The curve nearest the axis, is deduced from observations in which the aperture opposed to the tube was not greater than that of the tube itself; and shows what would be the diameter of the current, if the velocities of every one of its particles in the same circular section, including those of the contiguous air, which must have acquired as much motion as the current has lost, were equal among themselves. As the central particles must be supposed to be less impeded in their motion than the superficial ones, of course, the smaller the aperture opposed to the centre of the current, the greater the velocity ought to come out, and the ordinate of the curve the smaller; but, where the aperture was not greater than that of the tube, the difference of the velocities at the same distance was scarcely perceptible. When the aperture was larger than that of the tube, if the distance was very small, of course, the average velocity came out much smaller than that which was inferred from a smaller aperture; but, where the ordinate of the internal curve became nearly equal to this aperture, there was but little difference between the velocities indicated with different apertures. Indeed, in some cases, a larger aperture seemed to indicate a greater velocity: this might have arisen in some degree from the smaller aperture not having been exactly in the centre of the current; but there is greater reason to suppose, that it was occasioned by some resistance derived from the air returning between the sides of the aperture and the current entering it. Where this took place, the external curves, which are so constructed as that their ordinates are reciprocally in the subduplicate ratio of the pressure observed in the second cavity, with apertures equal in semidiameter to their initial ordinate, approach, for a short distance, nearer to the axis than the internal curve: after this, they continue their course very near to this curve. Hence it appears, that no observable part of the motion diverged beyond the limits of the solid which would be formed by the revolution of the internal curve, which is seldom inclined to the axis in an angle so great as ten degrees. A similar conclusion may be made, from observing the flame of a candle subjected to the action of a blowpipe: there is no divergency beyond the narrow limits of the current; the

flame, on the contrary, is every where forced by the ambient air towards the current, to supply the place of that which it has carried away by its friction. The lateral communication of motion, very ingeniously and accurately observed in water by Professor Venturi; is exactly similar to the motion here shown to take place in air; and these experiments fully justify him in rejecting the tenacity of water as its cause: no doubt it arises from the relative situation of the particles of the fluid, in the line of the current, to that of the particles in the contiguous strata, which is such as naturally lead to a communication of motion nearly in a parallel direction; and this may properly be termed friction. The lateral pressure which urges the flame of a candle towards the stream of air from a blowpipe; is probably exactly similar to that pressure which causes the inflection of a current of air near an obstacle. Mark the dimple which a slender stream of air makes on the surface of water; bring a convex body into contact with the side of the stream, and the place of the dimple will immediately show that the current is inflected towards the body; and, if the body be at liberty to move in every direction, it will be urged towards the current, in the same manner as, in Venturi's experiments, a fluid was forced up a tube inserted into the side of a pipe through which water was flowing. A similar interposition of an obstacle in the course of the wind, is probably often the cause of smoky chimneys. One circumstance was observed in these experiments, which it is extremely difficult to explain, and which yet leads to very important consequences: it may be made distinctly perceptible to the eye, by forcing a current of smoke very gently through a fine tube. When the velocity is as small as possible, the stream proceeds for many inches without any observable dilatation; it then immediately diverges at a considerable angle into a cone, Plate IV. Fig. 24; and, at the point of divergency, there is an audible and even visible vibration. The blowpipe also affords a method of observing this phenomenon: as far as can be judged from the motion of the flame, the current seems to make something like a revolution in the surface of the cone, but this motion is too rapid to be distinctly discerned. When the pressure is increased, the apex of the cone approaches nearer to the orifice of the tube, Figs. 25, 26; but no degree of pressure seems materially to alter its divergency. The distance of the apex from the orifice, is not proportional to the diameter of the current; it rather appears to be the greater, the smaller the current, and is much better defined in a small current than in a large one. Its distance in one experiment is expressed in Table x, from observations on the surface of a liquid; in other experiments, its respective distances were sometimes considerably less with the same degrees of pressure. It may be inferred, from the numbers of Tables VII and VIII, that in several instances a greater height of the first gage produced a less height of the second: this arose from the nearer approach of the apex of the cone to the orifice of the tube, the stream losing a greater portion of its velocity by this divergency than it gained by the increase of pressure. At first sight, the form of the current bears some resemblance to the *vena contracta* of a jet of water: but Venturi has observed, that in water an increase of pressure increases, instead of diminishing, the distance of the contracted

fection from the orifice. Is it not possible, that the facility with which some spiders are said to project their fine threads to a great distance, may depend upon the small degree of velocity with which they are thrown out, so that, like a minute current, meeting with little interruption from the neighbouring air, they easily continue their course for a considerable time?

Table v.

A	1.	2.	3.	3.8
B	C	C	C	C
1.	.1	.1	.1	
2.	.12	.12	.2	
3.	.17	.25	.3	
4.	.2	.4	.4	
5.	.25	.5		
6.	.30	.52		
7.	.35	.54	.5	
8.	.37	.56		
9.	.39	.58		
10.	.40	.6	.6	.5
15.		.7		
18.	.50			
20.				

The diameter of the tube .07. A is the distance of the liquid from the orifice. B, the pressure. C, the diameter of the surface of the liquid displaced.

Table vi.

A	1.	2.
B	C	C
1.	.1	.1
2.	.13	
3.	.2	.2
4.	.25	.3
6.	.3	.4
7.	.35	.5
10.	.35	.6
15.	.35	.7
20.	.35	.7

Diameter of the tube, .1.
A, B, and C, as in Table v.

Table vii.

A	.5	
B	.06	.15
C	D	D
.1	.083	
.2	.16	
.3	.25	.1
.4	.35	
.5	.45	
.6	.53	.2
.7	.6	
.8		.3
1.	.5	
1.2	.4	.4
1.5	.6	
2.	.67	.55
4.	1.3	1.
8.		2.
9.	.3	
14.	.5	

Diameter of the tube .06.

A is the distance of the opposite aperture, from the orifice of the tube. B, the diameter of the aperture. C, the pressure, indicated by the first gage. D, the height of the second gage.

Table VIII.

A	.5				1.				2.				4.			
B	.06	.15	.3	.5	.06	.15	.3	.5	.06	.15	.3	.5	.06	.15	.3	.5
C	D	D	D	D	D	D	D	D	D	D	D	D	D	D	D	D
.1	.05	.05			.03				.017							
.2	.1	.1			.12	.08	.02		.034							
.5	.2	.22			.1	.00			.00							
1.	.32	.36	.1		.17	.1	.1	.05	.04							
2.	.52	.6	.2		.28	.22	.21	.08	.07							
3.	.8	.9	.3		.4	.36	.32	.12	.12	.1	.1					
4.	1.1	1.2	.4		.58	.52	.42	.16	.18	.15	.14					
5.		1.5	.5		.8	.68	.52	.2	.23	.2	.18	.04		.04	.05	
6.		1.7	.6		1.	.83	.63	.25	.3	.25	.22	.05		.05	.06	
7.		1.9	.7		1.2	1.	.75	.3	.35	.3	.26	.06		.06	.07	
8.		2.1	.8		1.5	1.2	.88	.34	.4	.34	.3	.07		.07	.07	
9.		2.3	.9		1.7	1.4	1.	.37	.45	.37	.34	.08		.08	.08	
10.		2.6	1.		1.9	1.6	1.1	.4	.5	.4	.37	.09		.09	.09	

Diameter of the tube .i. A, B, C, and D, as in Table VII.

Table IX.

A	1.15				3.3			4.
B	.15	.3	.5	1.	.06	.15	1.	.06
C	D	D	D	D	D	D	D	D
.5	.1	.1	.1					
1.	.2	.2	.2					
2.	.4	.35	.34	.13	.1	.1	.125	
3.	.6	.5	.5	.2	.15	.15	.18	.1

Table x.

Diameter of the tube .3.	A	B	A is the pressure. B, the distance of the apex of the cone from the orifice of a tube .i in diameter.
A, B, C, and D, as in Table VII.	.4	6.	
	.8	3.	
	1.2	1.5	
	1.8	1.	
	2.	.5	
	4.	.0	

III. Ocular Evidence of the Nature of Sound.

A tube about the tenth of an inch in diameter, with a lateral orifice half an inch from its end, filed rather deeper than the axis of the tube, Fig. 27, was inserted at the apex of a conical cavity containing about twenty cubic inches of air, and luted perfectly tight: by blowing through the tube, a sound nearly in unison with the tenor C was produced. By gradually increasing the capacity of the cavity as far as several gallons, with the same mouth-piece, the sound, although faint, became more and more grave, till it was no longer a musical note. Even before this period a kind of trembling was distinguishable; and this, as the cavity was still further increased, was changed into a succession of distinct puffs, like the sound produced by an explosion of air from the lips; as slow, in some instances, as 4 or 3 in a second. These were undoubtedly the single vibrations, which, when repeated with sufficient frequency, impress on the auditory nerve the sensation of a continued sound. On forcing a current of smoke through the tube, the vibratory motion

of the stream, as it passed out at the lateral orifice, was evident to the eye; although, from various circumstances, the quantity and direction of its motion could not be subjected to exact mensuration. This species of sonorous cavity seems susceptible of but few harmonic sounds. It was observed, that a faint blast produced a much greater frequency of vibrations than that which was appropriate to the cavity: a circumstance similar to this obtains also in large organ pipes; but, several minute observations of this kind, although they might assist in forming a theory of the origin of vibrations, or in confirming such a theory drawn from other sources, yet, as they are not alone sufficient to afford any general conclusions, are omitted at present, for the sake of brevity.

(To be continued.)

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

Galvanic Pile consisting of one and the same Metal throughout.

MR. Humphry Davy, who is at present attached to the Royal Institution, has discovered, that the difference of the metals is not essential to the effect produced in the pile of Volta. From his former reasoning on these important phenomena, (see our Journal IV. 380, 394) he was led to make trial of powers by which that difference of oxydation at the surfaces, which appears to be essential to the effect, and has hitherto arisen from the habitudes of the metals themselves, might be produced with similar consequences by changing the solvents. The metals hitherto tried each to form a pile by itself, and as far as my recollection serves are silver, copper, zinc, and lead; and one of the arrangements of the chemical agents is thus; a plate of metal, cloth soaked in diluted nitrous acid, cloth soaked in water, cloth soaked in fulphuret of pot-ash; then another plate of the same metal, and the three cloths as before: in which manner the repetitions may be continued at pleasure. If a trough be used with cells, and the separation between the acid and the fulphuret of pot-ash be made by a plate of horn instead of the cloth imbibed with water, the two fluids may be connected by a slip of wetted cloth hung over the upper edge of the horn. This will complete the communication without occasioning any mixture, because water is lighter than either of the other fluids. It is remarkable that the effects of this pile are the reverse of what happens when the difference of action depends on the metals; that is to say, the extrication of hydrogen in the tube of communication, is made from the opposite wire to that from which it would have been made with the common pile.

Among the reflections and conclusions to which this valuable experiment must give rise, it is not one of the least important, that it overthrows the theory of the learned inventor Volta, who considers the current of electricity as being produced by the difference of conducting power in the different metals. And in this obscure region of research, though we are altogether unacquainted with the power by which the electricity is accumulated, it is one step towards a true theory to have ascertained, that one of those already offered is without foundation.

Philosophical

Philosophical Transactions of the Royal Society of London, for the Year 1800. Part III. Quarto, p. 295, with twelve Plates. London; sold by Elmsley. Price 17s. 6d.

THIS part contains :—19. Experiments on the solar, and on the terrestrial rays that occasion heat; with a comparative view of the laws to which light and heat, or rather the rays which occasion them, are subject, in order to determine whether they are the same or different. Part II. By William Herschel, L. L. D. F. R. S. 20. An Account of the Trigonometrical Survey, carried on in the years 1797, 1798, and 1799, by order of the Marquis Cornwallis, Master General of the Ordnance. By Captain William Mudge, of the Royal Artillery, F. R. S. Communicated by his Grace the Duke of Richmond, F. R. S.

Analytical Essays towards promoting the Chemical Knowledge of Mineral Substances. By Martin Henry Klaproth, Professor of Chemistry, Assessor to the Royal College of Physicians, Member of the Royal Academy of Sciences at Berlin, and various other learned Societies. Translated from the German. 1 vol. 8vo. p. 592. London. Sold by Cadell and Davies.

IT is altogether unnecessary to apprise the scientific world of the value of the analyses of this great improver of chemical science. I shall therefore only inform the reader in this place, that we are indebted for the present accurate and only translation to the learned editor of Gren's Chemistry, Dr. Gruber, who promises in his preface to give an immediate English edition of the next collection which Klaproth shall publish, and of which he has given hopes to the editor.

An Epitome of Chemistry in three Parts. Part I. intended to facilitate to the Student the Acquisition of Chemical Knowledge, by minute Instructions for the performance of Experiments. Part II. Directions for the Analysis of Mineral Waters; of Earths and Stones; of Ores of Metals; and of Mineral Bodies in general; and Part III. Instructions for applying Chemical Facts and reagents to various useful Purposes. By William Henry. Duodecimo, 216 Pages. Printed for Johnson.

THE title of this work is sufficiently copious, to exhibit its objects and plan. The practical, as well as scientific knowledge of the author, have enabled him to produce a manual which I doubt not will be received with avidity by the public. I will not incur the imputation of vanity by assuming the office of an approver, where the author's powers are so well known: but I think I shall not deviate from the line of my duty in observing that, as far as my observation extends, he has, on the present occasion, exercised those powers with the diligence and attention which are peculiarly demanded in a manual of science.

Electric

Electric Shock by means of Galvanism.

IN answer to a letter on the subject of the note at page 44, Mr. Cruickshank has favoured me with an account of the experiment in which the common electric jar was charged by Galvanism, so as to give a perceptible shock: a fact on the possibility of which all doubt has, with respect to myself, been removed by my first experiments with talc, with silk, and with common air; a plate of which, over the condenser of Volta, may be so charged with Galvanism, from a weak power, as to exhibit every electric intensity that Bennet's electrometer is capable of, merely by varying the thickness of the stratum of air.

Mr. Cruickshank made his experiment as follows: A coated jar, whose inside and outside coatings were moistened, was placed upon one of the extremities of the pile, and a wire connected with the inside coating, was then made to touch the other extremity. After this process the jar was taken in the hand which was wet, and the wire brought to touch the tongue. In this case, when the machine was very powerful, and gave strong sparks, he was several times confident that he received a slight shock in the tongue, but this did not always succeed, nor could he account for its failure. In truth, the reasoning on these facts by application of what we know of the laws of common electricity, is sufficiently obvious; but till we are better acquainted with the laws of electric condensation in the pile, we must expect anomalies tending to embarrass our processes of deduction. I have met with several; of which like Mr. Cruickshank, in the present instance, I have deferred the publication till time should admit of repeating them with such variations as the facts themselves have suggested.

From the same private communication I copy the following curious extract:—"I still use the new apparatus or troughs, which was described in your Journal, and have never once had occasion to remove the pieces. Indeed, when fresh filled with a solution of common salt, to which a little muriatic acid is added, it is quite as powerful as when first constructed. I prefer the muriatic acid to the nitrous, as its effects are much more permanent, and it does not in any sensible degree act on the silver. The shocks given by the troughs are exactly similar to those of common electricity, and have none of that grating or tearing sensation so remarkable in those from the pile. From two troughs, containing in all 120 pair of pieces, a spark of considerable size may be taken, and the report or snap may be heard in an adjoining room. The spark may likewise be taken from water, or from a conductor at a distance, which I never could do with any pile. If a point be presented to the water, a curious hissing is perceived, and a portion of the fluid is instantly converted into gas and vapour: I have sometimes dissipated a whole drop in this way. I have likewise seen a very strong spark run along a plate of zinc, to get to water in a manner which I cannot yet explain: this is frequently accompanied with a hissing noise. In short, I have observed a number of curious facts with this apparatus, which had escaped notice, when I employed the piles only."

Galvanic Apparatus.

Fig. 1.

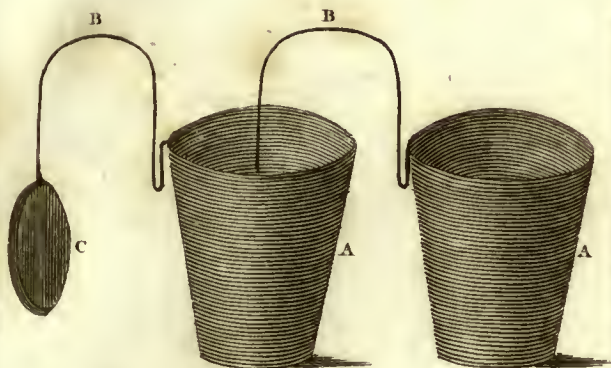


Fig. 3.



Fig. 2.

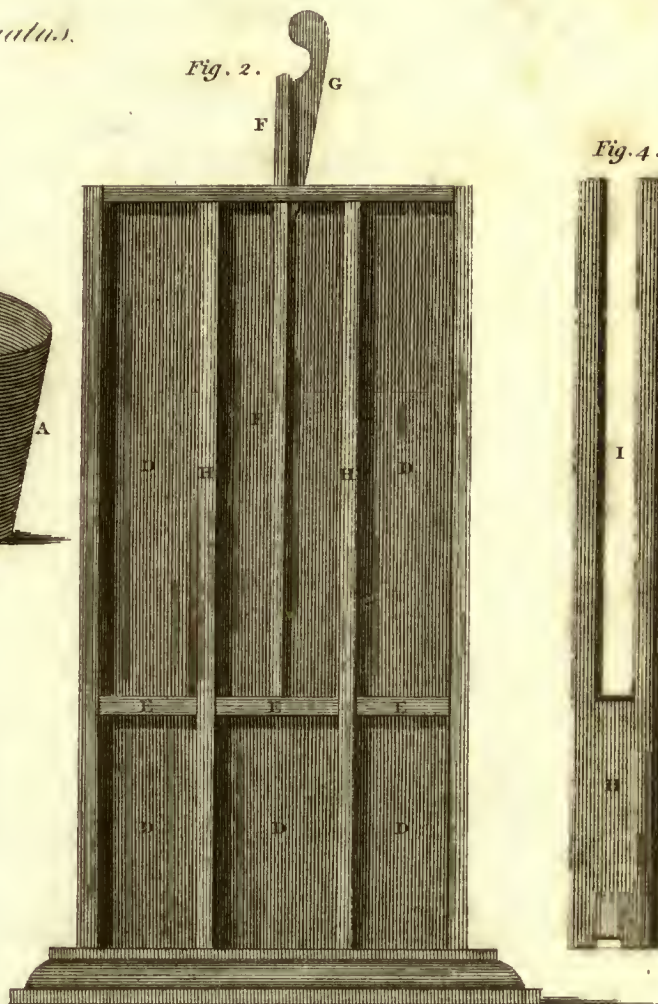
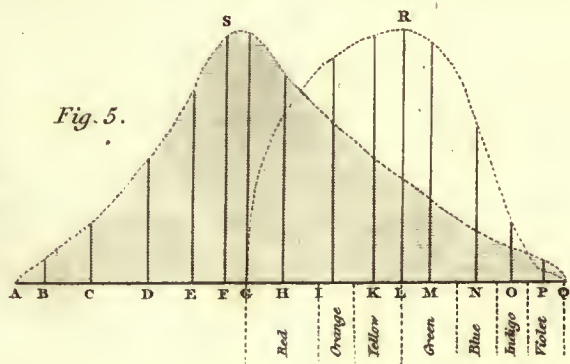


Fig. 4.

Dispersion & Intensity of solar radiant heat & Light

Fig. 5.





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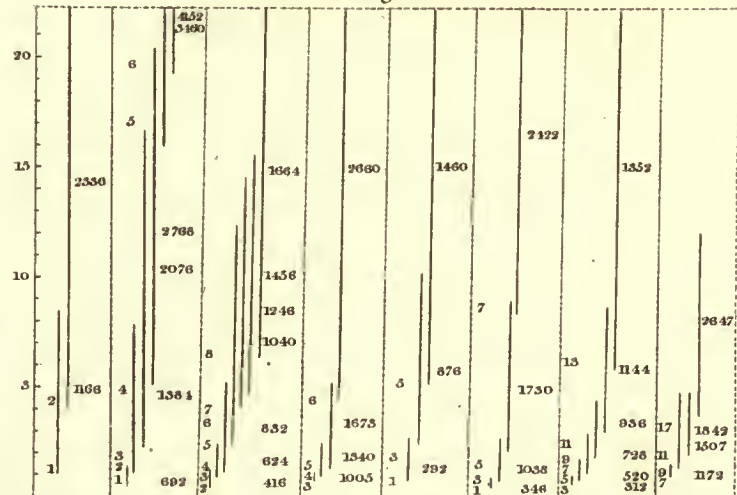
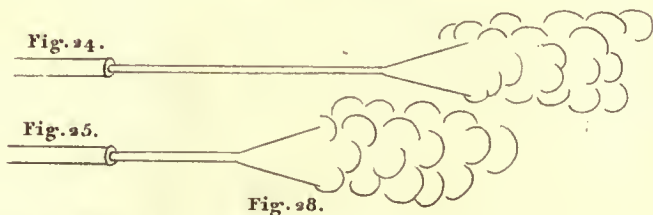
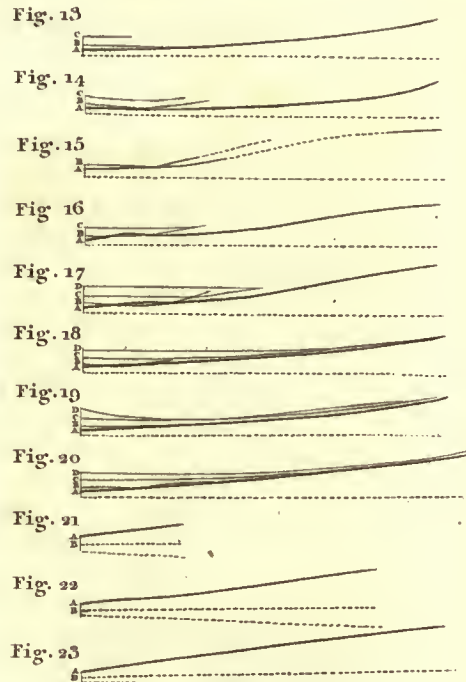
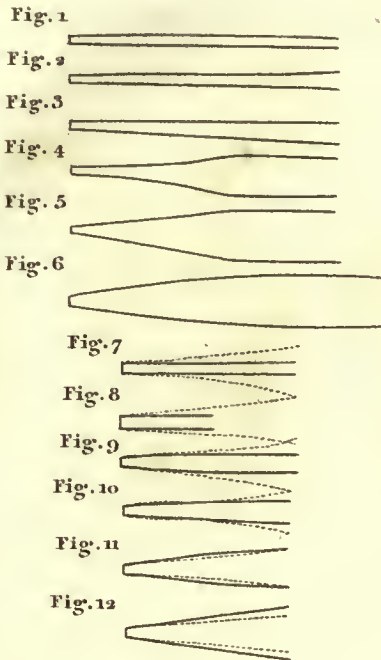
1932

1933

1934

1935

Dr. Young's Inquiries respecting Sound.



4.5 OPEN 8.4 OPEN 16.1 OPEN 20.5 OPEN 4.5 STOPPED 8.4 STOPPED 16.1 STOPPED 20.5 STOPPED



A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

JUNE, 1801.

ARTICLE I.

*Outlines of Experiments and Inquiries respecting Sound and Light. By THOMAS YOUNG,
M. D. F. R. S.*

(Continued from page 78.)

IV. *Of the Velocity of Sound.*

IT has been demonstrated, by M. de la Grange and others, that any impression whatever communicated to one particle of an elastic fluid, will be transmitted through that fluid with an uniform velocity, depending on the constitution of the fluid, without reference to any supposed laws of the continuation of that impression. Their theorem for ascertaining this velocity is the same as Newton has deduced from the hypothesis of a particular law of continuation: but it must be confessed, that the result differs somewhat too widely from experiment, to give us full confidence in the perfection of the theory. Corrected by the experiments of various observers, the velocity of any impression transmitted by the common air, may, at an average, be reckoned 1130 feet in a second.

V. Of sonorous Cavities.

M. de la Grange has also demonstrated, that all impressions are reflected by an obstacle terminating an elastic fluid, with the same velocity with which they arrived at that obstacle. When the walls of a passage, or of an unfurnished room, are smooth and perfectly parallel, any explosion, or a stamping with the foot, communicates an impression to the air, which is reflected from one wall to the other, and from the second again towards the ear, nearly in the same direction with the primitive impulse: this takes place as frequently in a second, as double the breadth of the passage is contained in 1130 feet; and the ear receives a perception of a musical sound, thus determined in its pitch by the breadth of the passage. On making the experiment, the result will be found accurately to agree with this explanation. If the sound is predetermined, and the frequency of vibrations such as that each pulse, when doubly reflected, may coincide with the subsequent pulse proceeding directly from the sounding body, the intensity of the sound will be much increased by the reflection; and also, in a less degree, if the reflected pulse coincides with the next but one, the next but two, or more, of the direct pulses. The appropriate notes of a room may readily be discovered by singing the scale in it; and they will be found to depend on the proportion of its length or breadth to 1130 feet. The sound of the stopped diapason pipes of an organ is produced in a manner somewhat similar to the note from an explosion in a passage; and that of its reed pipes to the resonance of the voice in a room: the length of the pipe in one case determining the sound, in the other, increasing its strength. The frequency of the vibrations does not at all immediately depend on the diameter of the pipe. It must be confessed, that much remains to be done in explaining the precise manner in which the vibration of the air in an organ pipe is generated. M. Daniel Bernouilli has solved several difficult problems relating to the subject; yet some of his assumptions are not only gratuitous, but contrary to matter of fact.

VI. Of the Divergence of Sound.

It has been generally asserted, chiefly on the authority of Newton, that if any sound be admitted through an aperture into a chamber, it will diverge from that aperture equally in all directions. The chief arguments in favour of this opinion are deduced from considering the phenomena of the pressure of fluids, and the motion of waves excited in a pool of water. But the inference seems to be too hastily drawn: there is a very material difference between impulse and pressure; and, in the case of waves of water, the moving force at each point is the power of gravity, which, acting primarily in a perpendicular direction, is only secondarily converted into a horizontal force, in the direction of the progress of the waves, being at each step disposed to spread equally in every direction: but the impulse transmitted by an elastic fluid, acts primarily in the direction of its progress. It is well known, that if a person calls to another with a speaking trumpet, he points it towards the
place:

place where his hearer stands : and I am assured by a very respectable Member of the Royal Society, that the report of a cannon appears many times louder to a person towards whom it is fired, than to one placed in a contrary direction. It must have occurred to every one's observation, that a sound such as that of a mill, or a fall of water, has appeared much louder after turning a corner, when the house or other obstacle no longer intervened; and it has been already remarked by Euler, on this head, that we are not acquainted with any substance perfectly impervious to sound. Indeed, as M. Lambert has very truly asserted, the whole theory of the speaking trumpet, supported as it is by practical experience, would fall to the ground, if it were demonstrable that sound spreads equally in every direction. In windy weather it may often be observed, that the sound of a distant bell varies almost instantaneously in its strength, so as to appear at least twice as remote at one time as at another; an observation which has also occurred to another gentleman, who is uncommonly accurate in examining the phenomena of nature. Now, if sound diverged equally in all directions, the variation produced by the wind could never exceed one-tenth of the apparent distance: but, on the supposition of a motion nearly rectilinear, it may easily happen that a slight change in the direction of the wind, may convey the sound, either directly or after reflection, in very different degrees of strength, to the same spot. From the experiments on the motion of a current of air, already related, it would be expected that a sound, admitted at a considerable distance from its origin through an aperture, would proceed, with an almost imperceptible increase of divergence, in the same direction; for, the actual velocity of the particles of air, in the strongest sound, is incomparably less than that of the slowest of the currents in the experiments related, where the beginning of the conical divergence took place at the greatest distance. Dr. Matthew Young has objected, not without reason, to M. Hube, that the existence of a condensation will cause a divergence in sound: but a much greater degree of condensation must have existed in the currents described than in any sound. There is indeed one difference between a stream of air and a sound; that, in sound, the motions of different particles of air are not synchronous: but it is not demonstrable that this circumstance would affect the divergency of the motion, except at the instant of its commencement, and perhaps not even then in a material degree; for, in general, the motion is communicated with a very gradual increase of intensity. The subject, however, deserves a more particular investigation; and, in order to obtain a more solid foundation for the argument, it is proposed, as soon as circumstances permit, to institute a course of experiments for ascertaining, as accurately as possible, the different strength of a sound once projected in a given direction, at different distances from the axis of its motion.

VII. *Of the Decay of Sound.*

Various opinions have been entertained respecting the decay of sound. M. de la Grange has published a calculation, by which its force is shown to decay nearly in the simple ratio of the distances; and M. Daniel Bernouilli's equations for the sounds of conical pipes lead to

M 2

a similar

a similar conclusion. The same inference would follow from a completion of the reasoning of Dr. Hellsam, Dr. Matthew Young, and Professor Venturi. It has been very elegantly demonstrated by Maclaurin, and may also be proved in a much more simple manner, that when motion is communicated through a series of elastic bodies increasing in magnitude, if the number of bodies be supposed infinitely great, and their difference infinitely small, the motion of the last will be to that of the first in the subduplicate ratio of their respective magnitudes; and, since in the case of concentric spherical laminæ of air, the bulk increases in the duplicate ratio of the distance, the motion will in this case be directly, and the velocity inversely, as the distance. But, however true this may be of the first impulse, it will appear, by pursuing the calculation a little further, that every one of the elastic bodies, except the last, receives an impulse in a retrograde direction, which ultimately impedes the effect of the succeeding impulse, as much as a similar cause promoted that of the preceding one: and thus, as sound must be conceived to consist of an infinite number of impulses, the motion of the last lamina will be precisely equal to that of the first; and, as far as this mode of reasoning goes, sound must decay in the duplicate ratio of the distance. Hence it appears, that the proposal for adopting the logarithmic curve for the form of the speaking trumpet, was founded on fallacious reasoning. The calculation of M. de la Grange is left for future examination; and it is intended, in the mean time, to attempt to ascertain the decay of sound as nearly as possible by experiment: should the result favour the conclusions from that calculation, it would establish a marked difference between the propagation of sound and of light.

VIII. *Of the harmonic Sounds of Pipes.*

In order to ascertain the velocity with which organ pipes of different lengths require to be supplied with air, according to the various appropriate sounds which they produce, a set of experiments was made, with the same mouth-piece, on pipes of the same bore, and of different lengths, both stopped and open. The general result was, that a similar blast produced as nearly the same sound as the length of the pipes would permit; or at least that the exceptions, though very numerous, lay equally on each side of this conclusion. The particular results are expressed in Table XI. and in Plate V. Fig. 28. They explain how a note may be made much louder on a wind instrument by a swell, than it can possibly be by a sudden impression of the blast. It is proposed, at a future time, to ascertain by experiment, the actual compression of the air within the pipe under different circumstances: from some very slight trials, it seemed to be nearly in the ratio of the frequency of vibrations of each harmonic.

Table

Table XI.

OPEN.						STOPPED.					
A	B	C	D	E	F	A	B	C	D	E	F
4.5		0.7	8.8	$\overline{d^*}$	1	4.5		0.3	1.8	\overline{d}	1
	4.1	8.8			2		1.2	1.7	10.0		3
							5.0	9.0			5
9.4		0.3	0.9	\overline{f}	1	9.4		0.2	0.4	\overline{f}	1
	0.8		8.0		2			0.45	1.6		3
	2.0		18.0		3		1.1	1.6	8.5		5
	5.0	8.0	20.0		4		7.0	8.0			7
	16.5	18.0			5						
	19.0	20.0			6						
16.1		0.4	1.0	$\overline{g^*}$	2	16.1		0.4	0.6	$\overline{d^*}$	3
	0.8	1.0	2.2		3		0.6	0.65	1.1		5
	1.2	2.2	4.7		4		0.9	1.1	2.4		7
	2.2	4.7	11.5		5		1.6	2.4	4.9		9
	3.4		13.5		6		2.5	4.8	9.0		11
	4.0		15.0		7		6.0	7.0			13
	6.5	10.0			8						
20.5		0.6	0.8	\overline{b}	3	20.5		0.8	1.1	$\overline{c^*}$	7
		0.8	1.9		4		1.0	1.1	3.8		9
	1.1	1.9	5.7		5		1.8		3.8		11
	4.5	5.7			8		3.2	3.8	12.	0	17
								12.			00

A, is the length of the pipe from the lateral orifice to the end. C, the pressure at which the sound began. B, its termination, by lessening the pressure; D, by increasing it. E, the note answering to the first sound of each pipe, according to the German method of notation. F, the number showing the place of each note in the regular series of harmonics. The diameter of the pipe was .35; the air duct of the mouth-piece measured, where smallest, .25 by .035; the lateral orifice .25 by .125. The apparatus was not calculated to apply a pressure of above 22 inches. Where no number stands under C, a sudden blast was required to produce the note.

IX. Of the Vibrations of different elastic Fluids.

All the methods of finding the velocity of sound, agree in determining it to be, in fluids of a given elasticity, reciprocally in the subduplicate ratio of the density: hence, in pure hydrogen gas it should be $\sqrt{13}=3.6$ times as great as in common air; and the pitch of a pipe should be a minor fourteenth higher in this fluid than in the common air. It is therefore

therefore probable that the hydrogen gas used in Professor Chladni's late experiments, was not quite pure. It must be observed, that in an accurate experiment of this nature, the pressure causing the blast ought to be carefully ascertained. There can be no doubt but that, in the observations of the French Academicians on the velocity of sound, which appear to have been conducted with all possible attention, the dampness and coldness of the night air must have considerably increased its density: hence, the velocity was found to be only 1109 feet in a second; while Derham's experiments, which have an equal appearance of accuracy, make it amount to 1142. Perhaps the average may, as has been already mentioned, be safely estimated at 1130. It may here be remarked, that the well known elevation of the pitch of wind instruments, in the course of playing, sometimes amounting to half a note, is not, as is commonly supposed, owing to any expansion of the instrument, for this should produce a contrary effect, but to the increased warmth of the air in the tube. Dr. Smith has made a similar observation, on the pitch of an organ in summer and winter, which he found to differ more than twice as much as the English and French experiments on the velocity of sound. Bianconi found the velocity of sound, at Bologna, to differ at different times, in the ratio of 152 to 157.

X. Of the Analogy between Light and Sound.

Ever since the publication of Sir Isaac Newton's incomparable writings, his doctrines of the emanation of particles of light from lucid substances, and of the formal pre-existence of coloured rays in white light, have been almost universally admitted in this country, and but little opposed in others. Leonard Euler indeed, in several of his works, has advanced some powerful objections against them, but not sufficiently powerful to justify the dogmatical reprobation with which he treats them; and he has left that system of an ethereal vibration, which after Huygens and some others he adopted, equally liable to be attacked on many weak sides. Without pretending to decide positively on the controversy, it is conceived that some considerations may be brought forwards, which may tend to diminish the weight of objections to a theory similar to the Huygenian. There are also one or two difficulties in the Newtonian system, which have been little observed. The first is, the uniform velocity with which light is supposed to be projected from all luminous bodies, in consequence of heat, or otherwise. How happens it that, whether the projecting force is the slightest transmission of electricity, the friction of two pebbles, the lowest degree of visible ignition, the white heat of a wind furnace, or the intense heat of the sun itself, these wonderful corpuscles are always propelled with one uniform velocity? For, if they differed in velocity, that difference ought to produce a different refraction. But a still more insuperable difficulty seems to occur, in the partial reflection from every refracting surface. Why, of the same kind of rays, in every circumstance precisely similar, some should be always reflected, and others transmitted, appears in this system to be wholly inexplicable. That a medium resembling, in many properties, that which has been denominated ether, does really exist, is undeniably proved by the phenomena of electricity;

electricity; and the arguments against the existence of such an ether throughout the universe, have been pretty sufficiently answered by Euler. The rapid transmission of the electrical shock, shows that the electric medium is possessed of an elasticity as great as is necessary to be supposed for the propagation of light. Whether the electric ether is to be considered as the same with the luminous ether, if such a fluid exists, may perhaps at some future time be discovered by experiment; hitherto I have not been able to observe that the refractive power of a fluid undergoes any change by electricity. The uniformity of the motion of light in the same medium, which is a difficulty in the Newtonian theory, favours the admission of the Huygenian; as all impressions are known to be transmitted through an elastic fluid with the same velocity. It has been already shown, that sound, in all probability, has very little tendency to diverge: in a medium so highly elastic as the luminous ether must be supposed to be, the tendency to diverge may be considered as infinitely small, and the grand objection to the system of vibration will be removed. It is not absolutely certain, that the white line visible in all directions on the edge of a knife, in the experiments of Newton and of Mr. Jordan, was not partly occasioned by the tendency of light to diverge. Euler's hypothesis, of the transmission of light by an agitation of the particles of the refracting media themselves, is liable to strong objections; according to this supposition, the refraction of the rays of light, on entering the atmosphere from the pure ether which he describes, ought to be a million times greater than it is. For explaining the phenomena of partial and total reflection, refraction, and inflection, nothing more is necessary than to suppose all refracting media to retain, by their attraction, a greater or less quantity of the luminous ether, so as to make its density greater than that which it possesses in a vacuum, without increasing its elasticity; and that light is a propagation of an impulse communicated to this ether by luminous bodies: whether this impulse is produced by a partial emanation of the ether, or by vibrations of the particles of the body, and whether these vibrations are, as Euler supposed, of various and irregular magnitudes, or whether they are uniform, and comparatively large, remains to be hereafter determined. Now, as the direction of an impulse transmitted through a fluid, depends on that of the particles in synchronous motion, to which it is always perpendicular, whatever alters the direction of the pulse, will inflect the ray of light. If a smaller elastic body strike against a larger one, it is well known that the smaller is reflected more or less powerfully, according to the difference of their magnitudes: thus, there is always a reflection when the rays of light pass from a rarer to a denser stratum of ether; and frequently an echo when a sound strikes against a cloud. A greater body striking a smaller one, propels it, without losing all its motion: thus, the particles of a denser stratum of ether, do not impart the whole of their motion to a rarer, but, in their effort to proceed; they are recalled by the attraction of the refracting substance with equal force; and thus a reflection is always secondarily produced, when the rays of light pass from a denser to a rarer stratum. Let AB, Plate V. Fig. 29, be a ray of light falling on the reflecting surface FG; *c d* the direction of the vibration, pulse, impression, or condensation.

fation. When d comes to H , the impression will be, either wholly or partly, reflected with the same velocity as it arrived, and EH will be equal to DH ; the angle EIH to DIH or CIF ; and the angle of reflection to that of incidence. Let FG , Fig. 30, be a refracting surface. The portion of the pulse IE , which is travelling through the refracting medium, will move with a greater or less velocity in the subduplicate ratio of the densities, and HE will be to KI in that ratio. But HE is, to the radius IH , the sine of the angle of refraction; and KI that of the angle of incidence. This explanation of refraction is nearly the same as that of Euler. The total reflection of a ray of light by a refracting surface, is explicable in the same manner as its simple refraction; HE , Fig. 31, being so much longer than KI , that the ray first becomes parallel to FG , and then, having to return through an equal diversity of media, is reflected in an equal angle. When a ray of light passes near an inflecting body, surrounded, as all bodies are supposed to be, with an atmosphere of ether denser than the ether of the ambient air, the part of the ray nearest the body is retarded, and of course the whole ray inflected towards the body, Fig. 32. The repulsion of inflected rays has been very ably controverted by Mr. Jordan, the ingenious author of a late publication on the Inflection of Light. It has already been conjectured by Euler, that the colours of light consist in the different frequency of the vibrations of the luminous ether: it does not appear that he has supported this opinion by any argument; but it is strongly confirmed, by the analogy between the colours of a thin plate and the sounds of a series of organ pipes. The phenomena of the colours of thin plates require, in the Newtonian system, a very complicated supposition, of an ether, anticipating by its motion the velocity of the corpuscles of light, and thus producing the fits of transmission and reflection; and even this supposition does not much assist the explanation. It appears, from the accurate analysis of the phenomena which Newton has given, and which has by no means been superseded by any later observations, that the same colour recurs whenever the thickness answers to the terms of an arithmetical progression. Now this is precisely similar to the production of the same sound, by means of an uniform blast, from organ-pipes which are different multiples of the same length. Supposing white light to be a continued impulse or stream of luminous ether, it may be conceived to act on the plates as a blast of air does on the organ-pipes, and to produce vibrations regulated in frequency by the length of the lines which are terminated by the two refracting surfaces. It may be objected that, to complete the analogy, there should be tubes, to answer to the organ-pipes: but the tube of an organ-pipe is only necessary to prevent the divergence of the impression, and in light there is little or no tendency to diverge; and indeed, in the case of a resonant passage, the air is not prevented from becoming sonorous by the liberty of lateral motion. It would seem, that the determination of a portion of the track of a ray of light through any homogeneous stratum of ether, is sufficient to establish a length as a basis for colorific vibrations. In inflections, the length of the track of a ray of light through the inflecting atmosphere may determine its vibrations: but, in this case, as it is probable that there is a reflection from every part of the surface of the surrounding atmosphere, contributing

tributing to the appearance of the white line in every direction, in the experiments already mentioned; so it is possible that there may be some second reflection at the immediate surface of the body itself, and that, by mutual reflections between these two surfaces, something like the anguiform motion suspected by Newton may really take place; and then the analogy to the colours of thin plates will be still stronger. A mixture of vibrations, of all possible frequencies, may easily destroy the peculiar nature of each, and concur in a general effect of white light. The greatest difficulty in this system is, to explain the different degree of refraction of differently coloured light, and the separation of white light in refraction: yet, considering how imperfect the theory of elastic fluids still remains, it cannot be expected that every circumstance should at once be clearly elucidated. It may hereafter be considered how far the excellent experiments of Count Rumford, which tend very greatly to weaken the evidence of the modern doctrine of heat, may be more or less favourable to one or the other system of light and colours. It does not appear that any comparative experiments have been made on the inflection of light by substances possessed of different refractive powers; undoubtedly some very interesting conclusions might be expected from the inquiry.

XI. Of the Coalescence of Musical Sounds.

It is surprising that so great a mathematician as Dr. Smith could have entertained for a moment, an idea that the vibrations constituting different sounds should be able to cross each other in all directions, without affecting the same individual particles of air by their joint forces: undoubtedly they cross, without disturbing each other's progress; but this can be no otherwise effected than by each particle partaking of both motions. If this assertion stood in need of any proof, it might be amply furnished by the phenomena of beats, and of the grave harmonics observed by Romieu and Tartini; which M. de la Grange has already considered in the same point of view. In the first place, to simplify the statement, let us suppose, what probably never precisely happens, that the particles of air, in transmitting the pulses, proceed and return with uniform motions; and, in order to represent their position to the eye, let the uniform progress of time be represented by the increase of the absciss, and the distance of the particle from its original position, by the ordinate, Fig. 33—38. Then, by supposing any two or more vibrations in the same direction to be combined, the joint motion will be represented by the sum or difference of the ordinates. When two sounds are of equal strength, and nearly of the same pitch, as in Fig. 36, the joint vibration is alternately very weak and very strong, producing the effect denominated a beat, Plate VI. Fig. 43, B and C: which is slower and more marked, as the sounds approach nearer to each other in frequency of vibrations; and, of these beats there may happen to be several orders, according to the periodical approximations of the numbers expressing the proportions of the vibrations. The strength of the joint sound is double that of the simple sound only at the middle of the beat, but not throughout its duration; and it may be inferred, that the strength of sound in a concert will not be in

exact proportion to the number of instruments composing it. Could any method be devised for ascertaining this by experiment, it would assist in the comparison of sound with light. In Plate V. Fig. 33, let P and Q be the middle points of the progress or regress of a particle in two successive compound vibrations; then, CP being = PD, KR = RN, GQ = QH, and MS = SO, twice their distance, $2RS = 2RN + 2NM + 2MS = KN + NM + NM + MO = KM + NO$, is equal to the sum of the distances of the corresponding parts of the simple vibrations. For instance, if the two sounds be as 80 : 81, the joint vibration will be as 80.5; the arithmetical mean between the periods of the single vibrations. The greater the difference in the pitch of two sounds, the more rapid the beats, till at last, like the distinct puffs of air in the experiments already related, they communicate the idea of a continued sound; and this is the fundamental harmonic described by Tartini. For instance, in Plate V. Fig. 34—37, the vibrations of sounds related as 1 : 2, 4 : 5, 9 : 10, and 5 : 8, are represented; where the beats, if the sounds be not taken too grave, constitute a distinct sound, which corresponds with the time elapsing between two successive coincidences, or near approaches to coincidence: for, that such a tempered interval still produces a harmonic, appears from Plate V. Fig. 38. But, besides this primary harmonic, a secondary note is sometimes heard, where the intermediate compound vibrations occur at a certain interval, though interruptedly; for instance, in the coalescence of two sounds related to each other as 7 : 8, 5 : 7, or 4 : 5, there is a recurrence of a similar state of the joint motion, nearly at the interval of $\frac{5}{13}$, $\frac{4}{11}$, or $\frac{3}{9}$ of the whole period: hence, in the concord of a major third, the fourth below the key note is heard as distinctly as the double octave, as is seen in some degree in Plate V. Fig. 35; AB being nearly two-thirds of CD. The same sound is sometimes produced by taking the minor sixth below the key note; probably because this sixth, like every other note, is almost always attended by an octave, as a harmonic. If the angles of all the figures resulting from the motion thus assumed be rounded off, they will approach more nearly to a representation of the actual circumstances; but, as the laws by which the motion of the particles of air is regulated, differ according to the different origin and nature of the sound, it is impossible to adapt a demonstration to them all: if, however, the particles be supposed to follow the law of the harmonic curve, derived from uniform circular motion, the compound vibration will be the harmonic instead of the arithmetical mean; and the secondary sound of the interrupted vibrations will be more accurately formed, and more strongly marked Plate VI. Figs. 41, 42: the demonstration is deducible from the properties of the circle. It is remarkable, that the law by which the motion of the particles is governed, is capable of some singular alterations by a combination of vibrations. By adding to a given sound other similar sounds, related to it in frequency as the series of odd numbers, and in strength inversely in the same ratios, the right lines indicating an uniform motion may be converted very nearly into figures of sines, and the figures of sines into right lines, as in Plate V. Figs. 39, 40.

XII. *Of the Frequency of Vibrations constituting a given Note.*

The number of vibrations performed by a given sound in a second, has been variously ascertained; first, by Sauveur, by a very ingenious inference from the beats of two sounds; and since, by the same observer and several others, by calculation from the weight and tension of a chord. It was thought worth while, as a confirmation, to make an experiment suggested, but coarsely conducted, by Merfennus, on a chord 200 inches in length, stretched so loosely as to have its single vibrations visible; and, by holding a quill nearly in contact with the chord, they were made audible, and were found, in one experiment, to recur 8.3 times in a second. By lightly pressing the chord at one-eighth of its length from the end, and at other shorter aliquot distances, the fundamental note was found to be one-sixth of a tone higher than the respective octave of a tuning-fork marked C: hence, the fork was a comma and a half above the pitch assumed by Sauveur, of an imaginary C, consisting of one vibration in a second.

(To be concluded in our next.)

II.

On the Identity of the Pyro-mucous, Pyro-tartareous, and Pyro-ligneous Acids, with the Acetous Acid. By Citizens FOURCROY and VAUQUELIN.*

THE re-union of species which have been erroneously considered as different, is in many instances of greater advantage to the science of facts, than the discovery of new species. It is towards this degree of perfection that the efforts of chemists and naturalists are at present directed.

Three acids have been distinguished by different appellations, in which the common property of being produced by the action of fire, and of acquiring several common characters from that agent, is well known; but it was never imagined that they are not only identically the same, but likewise analogous in their nature to the acid of vinegar, denominated acetous acid. Citizens Fourcroy and Vauquelin, in their endeavours to discover certain characters by which the pyro-mucous, pyro-tartareous, and pyro-ligneous acids might be distinguished, were unable to discover any. By comparing those which have hitherto been pointed out as the distinctive characters, it may be shewn, as they have done, that they are founded merely on slight differences of smell and colour; and are consequently delusive.

* Bulletin de la Soc. Philom.---N.

Some facts observed in the analyses of vegetables, led our chemists to suspect that these acids, which were proved to be the same, might not differ from the acetous acid, except by foreign additions. They made experiments with a view to confirm their suspicions. By the distillation of sugar, starch, gum, &c. they obtained the pyro-mucous acid; by that of tartar, the pyro-tartareous acid; and by that of wood, the pyro-ligneous acid. They combined these acids with an alkaline basis; from which, having separated them by the sulphuric acid, they obtained by this means, in some instances, with filtration through charcoal powder, an acid and limpid liquor, deprived of the empyreumatic oil, which rendered it impure, and prevented it from being distinguished as the true acetous acid. This liquor then possessed the smell, taste, and all the other characters; and, with the alkaline bases, formed perfect acetites.

It was necessary to attempt to form the empyreumatic acids again by the acetous acid, by imparting to the latter the empyreumatic oil and empyreumatic smell, of which it had been deprived by the preceding experiments. The following is the process employed by Citizens Fourcroy and Vauquelin:—They distilled a portion of acetous acid over the empyreumatic oils of mucilage, tartar, and wood; and thus formed pyro-mucous, pyro-tartareous, and pyro-ligneous acids. To form these acids again instantaneously, it was only requisite to add a few drops of those empyreumatic oils to some acetous acid.

To these facts, which are sufficiently interesting in themselves, the discoverers have subjoined reflexions no less important, relative to the frequent production of the acetous acid under very different circumstances.

The acetous acid is not therefore a necessary product of the vinous fermentation. In addition to the facts above related, which confirm the production of this acid without any fermentation, it may be observed, that many substances, of a nature very different from wine, contain acetites; such are the sap of vegetables kept only a few hours, soils, heated tan, the sour water of the starch matter, sour pulse, and sour fruit, exposed for some hours to a hot air. Milk, animal jellies, and that part of urine called *urée*, when they become sour, likewise afford a great quantity of this acid.

Citizens Fourcroy and Vauquelin have discovered four very distinct circumstances under which the acetous acid is produced, each of which is characterised by some particulars relative to the phenomena of its production.

The first mode of *acetification* is the decomposing action of fire: in this case water is disengaged with carbonic acid formed from the disengaged carbon; and the acetous acid thus obtained is distinguished by its colour and empyreumatic odour.

The second method consists in the application of the powerful acids, such as the sulphuric, nitric, and the oxygenated muriatic acids upon vegetable compounds, such as sugar, jelly, &c.; alcohol and the vegetable acids themselves, except the acetous acid, are entirely decomposed by these acids. Some water and carbonic acid are also produced, and the acetous acid thus obtained contains oxalic acid, malic acid, and much water.

The

The third and most general method is the fermentation of wine. In this process neither water nor carbonic acid is produced; but an absorption of oxygen takes place: this acetous acid contains tartar, alcohol, and colouring matter.

The fourth mode, which is nearly similar to the third, the difference, perhaps, being only because it is no longer the vegetable fluid called *wine* which produces the acetous acid, is a particular fermentation which takes place in certain animal or vegetable products above-mentioned, but especially in urines. This acid is always combined with ammonia.

III.

*On the Structure and Uses of the Membrana Tympani of the Ear. By EVERARD HOME, Esq. F. R. S. **

THE subject of inquiry appointed by the Croonian Institution, has been greatly elucidated at different times by ingenious members of this learned Society. A large field, however, still remained open; and, respecting future investigations, I shall have occasion to offer a fresh proof of the aid to be derived from comparative anatomy, in ascertaining the structure of parts which, from their minuteness and situation in the human body admit with much difficulty of being explored.

The principal object of the present lecture is to communicate a discovery of the structure of the *membrana tympani*; which, in some respects, affords a new and very curious instance of the application of muscular action, and may conduce to account for certain phenomena in the sense of hearing, in a more satisfactory manner than has hitherto been proposed.

The *membrana tympani* has always been considered as a common membrane, which, by means of muscles belonging to the malleus being stretched or relaxed, became fitted, in its various degrees of tension, to convey the vast variety of external sounds to the internal organ. Its shape, situation, and office, have procured it the name of drum of the ear; and the muscles of the malleus having been deemed sufficient for bracing and unbracing it, less attention was bestowed on the structure of the membrane itself: to which may be added, that in the human ear, and generally in the ear of quadrupeds, the membrane is so extremely small and thin, and in its situation so peculiarly confined, as not to be got at for inspection but with much difficulty.

The case is different in the elephant, where this membrane is so very large, that the parts of which it is composed are readily distinguished: they are even conspicuous to the naked eye; and muscular fibres are seen passing along the membrane, in a radiated manner, from the bony rim which surrounds it, towards the handle of the malleus, to which the central part of the membrane is firmly attached.

* *Philos. Transactions*, 1800.

This discovery in the elephant having led to that of a similar construction in the human *membrana tympani*, it may not be improper to relate the circumstances by which I became engaged in the investigation of the organ of hearing in that animal.

Three different opportunities have occurred of dissecting the elephant in London, by the deaths of those which had been presented to his Majesty, and were kept at the King's stables at Pimlico. One of them was given to the late Dr. Hunter; one to his brother Mr. J. Hunter; and the third to Sir Ashton Lever.

From my being connected with Mr. John Hunter's pursuits in comparative anatomy, I was employed throughout the whole of these dissections, and became extremely desirous of examining the internal parts of the ear, the structure of that organ in the human body having at a very early period particularly engaged my attention;* but neither Dr. Hunter nor his brother could be prevailed upon to sacrifice so large a portion of the skull as was necessary for the purpose.

When Mr. Corfe arrived from Bengal, last year, and mentioned his having brought over a number of skulls of elephants, in order to show the progress of the formation of their grinding teeth, † the desire to examine the organ of hearing in that animal recurred to me so strongly, that I requested to have one of the skulls for that purpose, and Mr. Corfe very readily and obligingly complied with my request.

After having examined the organ in the dried skull, the want of the *membrana tympani*, and of the small bones, made the information thus received of a very unsatisfactory nature, and increased the desire of seeing these parts in the recent head. In considering how this could be done, I recollected a mutilated elephant's head, preserved in spirits, which had been sent to Mr. Hunter, but, from the multiplicity of his engagements, had remained neglected in the cask at the time of his death, and in the following year was dried, to show the proboscis, that it might not be altogether spoiled.

Upon examining this dried head, the bones had been so much broken, that one of the organs of hearing was altogether wanting: the other, however, was fortunately entire; and the *membrana tympani* and small bones, having been little disturbed in the drying of the parts, remained nearly in their natural situation.

* In the year 1776, I injected the cochlea and semicircular canals of the human ear with a composition of wax and rosin. This was done by placing the temporal bone in the receiver of an air pump, the upper part of which was in the form of a funnel, rendered air-tight by a cork being fitted into its neck, and surrounded with bees' wax. After the air had been exhausted, the hot injection, poured into the funnel, melted the wax, and the cork was pulled out by means of a string previously attached to it; the injection immediately rushed into the receiver, and was forced, by the pressure of the atmosphere, into the cavities of the temporal bone.

† On this subject, a very ingenious paper has been since published by him, in the *Philosophical Transactions* for the year 1799.

The membrana tympani, and every other part of the organ, were found to be much larger in proportion than in other quadrupeds, or in man; differing in this respect from the eye of the elephant, which is unusually small, when compared with the enormous bulk of the animal.

The membrane was found of an oval form; the short diameter of the oval rather more than an inch in length; the long diameter an inch and $\frac{7}{10}$ ths.

In the human ear, the membrana tympani is nearly circular; the longest diameter is $\frac{8}{10}$ ths of an inch; the shortest $\frac{7}{10}$ ths.

As the membrane in the elephant exceeds that of the human ear in thickness as much as in extent, which is as the squares of their diameters, or in the proportion of 135 to 14, it is natural to conclude that the muscular fibres which are to stretch the one, must greatly exceed in strength those capable of producing the same degree of tension in the other.

From this statement, the muscular structure in the human membrana tympani will necessarily be so much less distinct than in the elephant, as scarcely to be visible to the naked eye, and will easily be overlooked by the most attentive observer, who is not directed by some previous information to examine it under the most favourable circumstances; but, when these are attended to, it can be perceived without the aid of glasses.

If the membrana tympani of the human ear is completely exposed on both sides, by removing the contiguous parts, and the circular covering is carefully washed off from its external surface, then, by placing it in a clear light, the radiated direction of its fibres may be easily detected. If a common magnifying glass is used, they are rendered nearly as distinct as those of the elephant appear to the naked eye; their course is exactly the same; and they differ in nothing but in being formed upon a smaller scale.

When viewed in a microscope magnifying 23 times, the muscular fibres are beautifully conspicuous, and appear uniformly the same throughout the whole surface, there being no central tendons, as in the diaphragm; the muscular fibres appear only to form the internal layer of the membrane, and are most distinctly seen when viewed on that side.

In examining this membrane in different subjects, the parts were frequently found in a more or less morbid state. In one instance, the membrane was found loaded with blood-vessels, was less transparent than usual, and was united by close adhesion to the point of the long process of the incus. In another instance, there was a preternatural ossification adhering to it, at a small distance from the end of the handle of the malleus.

As muscles in general are supplied with blood-vessels in proportion to the frequency of their action, it is an object of importance to determine the vascularity of the membrana tympani. Upon this subject, my own want of information has been amply supplied by Dr. Baillie, who, in a communication upon this subject, showed me a preparation of the membrane, in which the vessels had been most successfully injected with coloured wax.

In this preparation, the most beautiful of the kind I ever saw, the vessels in their distribution resembled those of the iris, and were nearly half as numerous: they anastomosed with one another in a similar manner, and their general direction was from the circumference

circumference to the handle of the malleus; from near this handle, a small trunk sent off branches, in a radiated manner, which anastomosed with those which had an opposite course.

This correspondence, in the number and distribution of blood vessels, between the membrana tympani and the iris, is a strong circumstance in confirmation of that membrane being endowed with muscular action.

In the horse, the membrana tympani is smaller than in man; its long diameter is $\frac{8}{10}$ ths of an inch; the short one $\frac{6}{10}$ ths; and it is almost quite flat, while in man it is concave, which makes the difference of extent considerably exceed the difference in the diameters. In the horse, the fibrous structure is not visible to the naked eye; it is even indistinctly seen when viewed through a common magnifying glass; but in a microscope it is very visible, and in every other respect agrees in structure with the membrane in the human ear, and in that of the elephant.

In birds, the membrana tympani is larger in proportion than in the quadruped, and more circular in its shape.

In the goose, it is $\frac{6}{10}$ ths of an inch in its longest diameter, and $\frac{5}{10}$ ths in its shortest diameter. In the turkey, $\frac{7}{10}$ ths by $\frac{5}{10}$ ths. It is thinner in its coats in birds than in the horse, and to the naked eye has no appearance of fibres; but, when viewed in a microscope, there is a visible radiated structure, not very unlike the wire marks upon common writing paper.

In a former Lecture upon the Structure of Muscles,* in which a comprehensive view was taken of the subject, it was stated, that the organization necessary for muscular contraction could exist in an apparent membrane, and that a fasciculated structure was only necessary when muscular action was to be enabled to overcome resistance. The coats of the *Tænia hydatigena* were mentioned as an instance of the first; and the human heart as the most complex of the second. In comparing the membranæ tympani of different animals, they afford a beautiful illustration of the truth of this position.

In birds, where from the smallness of its size the resistance is very trifling, the membrane is very similar to the coat of an hydatid, only still thinner. In the elephant, fibres forming fasciculi are very distinct. The membrane of the horse, and that of the human ear, form the intermediate gradations.

The knowledge of a muscular structure in the membrana tympani, enables us to explain many phænomena in hearing, which have not hitherto been accounted for in a satisfactory manner. It is principally by means of this muscle that accurate perceptions of sound are communicated to the internal organ, and that the membrana tympani is enabled to vary the state of its tension, so as to receive them in the quick succession in which they are conveyed to it.

* Philosophical Transactions for the year 1795.

In the human ear, and in that of birds, the radiated fibres of the membrana tympani have their principal attachment to the extremity of the handle of the malleus, which is nearly in the centre of the membrane.

In the membrane of the elephant, which is oval, the attachment to the handle of the malleus is at some distance from the centre. In the horse, deer, and cat, which have the membrane still more oval than the elephant, the handle of the malleus is situated in the long axis of the membrane, with its extremity extending beyond the centre, reaching nearer to the circumference; and the fibres of the radiated muscle are not only attached to its end, but also laterally to nearly the whole length of its handle.

This oval form of the membrana tympani, in those quadrupeds, and the very extensive attachment of the fibres of the radiated muscle to the handle of the malleus, may be the reason why their ears are not equally fitted to hear inarticulate sounds, as the ears of birds and of man.

Should this radiated muscle of the membrana tympani (which is probably the smallest in the body that has a distinct action) be thought too insignificant to have an office of so much consequence assigned to it, let it be remembered, that the size of muscles is no indication of their importance, but only of the resistance to be overcome by their action; and that the more delicate actions are performed universally in the body by very small muscles, of which the iris in the eye furnishes a very conspicuous example.

(To be continued.)

IV.

Inquiries relative to the Laws of Affinity, by CIT. BERTHOLLET, Member of the National Institute of Arts and Sciences.*

(Concluded from page 69.)

ARTICLE XIII.

On the Precipitation of Metallic Solutions by other Metals.

1. **W**HEN the metals are precipitated by a substance which does not seize their oxygen, the precipitates retain part of the acid, and often part of the precipitant.

We have a striking instance of this division, which then takes place in the precipitate of the oxygenated muriate of mercury, by the fixed alkalis, ammonia and lime. On exposing the precipitate to a sufficient heat, a more or less considerable portion of mercury, according to the nature of the precipitate, is reduced, another is sublimed, and forms a muriate, not because the muriatic acid is found combined only with a portion of the oxide of mer-

* Translation from the *Annales de Chimie*, XXXVII. 151.

cury, as I supposed, (*Memoirs of the Academy*, 1780) but because the expansive force of the heat, and the tendency to the combination of the muriatic acid, acting upon the oxide of mercury, cause, as we may express ourselves, a new sharing to be made of it. If the precipitate by ammonia be examined, it is observed, that it retains a portion of ammonia: the precipitate of the muriate of iron by pot-ash, retains part of the pot-ash. Facts of this kind may be collected in great number.

There is therefore no doubt, but that the observations which have been made upon the precipitations of substances which lose their solubility, cannot be applied to the metallic precipitates, which vary according to all the circumstances which are capable of modifying the powers brought in action, at the moment of their successive precipitation, and which will deserve a particular examination in another memoir.

But when the metals are mutually precipitated from their solutions, their reciprocal affinity for oxygen has a great effect in the action that takes place; and sometimes the precipitate is found in the metallic state. If another force were not joined to the affinity of the precipitating metal for oxygen, it would naturally follow from the principles established in this Memoir, that the oxygen would always be divided between the two metals which are in competition, according to the action they exert upon it. It is therefore necessary to examine, what may be the force which determines the precipitation in the metallic state?

2. The affinity of mercury, gold, and silver for oxygen is very weak: the mutual affinity which still remains between the parts of these metals when they are in fusion, as mercury is at the temperature of the atmosphere, is sufficient to prevent their combination with oxygen in the state of gas; but heat, by dilating the particles of the mercury, sufficiently diminishes the force of their mutual affinity to enable them to combine with oxygen: a superior degree of heat is sufficient, by the difference of dilatation which it produces in the mercury and in the oxygen, to separate them: so that the action of the heat begins by being more effective to this combination, in consequence of the diminution of the mutual affinity of the parts, than inimical to it by the different dilatation of the mercury and the oxygen: but at length by augmenting this difference, it renders their combination impossible.

As the force of cohesion in mercury is sufficient to prevent oxidation, this very force will tend to effect its disoxygenation, with the assistance of a metal which will act directly upon the oxygen. It is a force analogous to that which produces crystallisation and precipitations. (Art. V.)

3. The metallic particles have not only a mutual affinity, but likewise have an affinity for those of other metals: hence proceed the amalgams and mixtures. It is only necessary to put copper in contact with mercury to effect a combination between these two metals. Thus when a metal opposes its action to a metallic solution, part of the metal may act upon the oxygen and upon the acid, while the other tends to combine with the metal of the oxide. Let us examine, whether we can discover in the precipitation of mercury, silver
gold

gold and copper, in the metallic state, the influence of these two forces, that is to say, the mutual affinity of the particles of one and the same metal, and the affinity of one metal for another.

4. When a piece of copper is plunged in a solution of mercury by the nitric acid, or by the muriatic acid, the copper becomes instantly white, and the mercury is found to be reduced; but it has combined with the copper.

If instead of a plate of copper, a plate of very clean iron be plunged in the same solution, several hours elapse before the liquid appears disturbed, and a precipitate is perceived: at length a precipitate is formed; but with the muriatic solution in particular, this precipitate is partly in the state of an oxide, and very probably retains a portion of acid.

If the affinity of a metal for oxygen were the only cause which produced the precipitation of another metal, the iron ought to act with much greater efficacy than the copper; for it is known to have a much stronger affinity for oxygen, and nevertheless its action is slow, difficult and incomplete, while that of copper is instantaneous. It is seen by the manner in which the indecomposable acids are retained by the oxide of copper and the oxide of iron, when their combination is exposed to the action of heat, that there can only be a very small difference between the affinities of these metals for the acids. There is consequently no doubt that the affinity of the copper for the mercury, with which it has actually combined, must have greatly contributed to its precipitation in the metallic state; but the mutual affinity of the particles of mercury, must alone have decided, though with difficulty, in the experiment with iron, the reduction of the former metal; a portion was also precipitated in oxide, and probably retained a portion of acid, as the whole would have done, if the affinity of the iron for oxygen had been the sole agent; and the portion precipitated in the metallic state did not combine with the iron.

5. When a solution of silver is precipitated by copper, the precipitate which is found in the metallic state is not of pure silver, but a combination of silver with a small portion of copper: it could not take the copper from the plate itself which was plunged in the solution; it must therefore have precipitated with it out of the solution: the mutual affinity of these two metals has decided their disoxygenation. By means of this force two combinations are effected, as happens also in several other circumstances: one of the acids, with the oxide of copper, the other of silver with a portion of the copper. The action of the acid upon the oxide of copper, and that of silver upon copper, are thus put in equilibrio.

6. So likewise when a plate of copper is plunged into a solution of gold, the gold which is precipitated shews by its higher colour, that it has combined with copper, and the solution retains but a small part of the copper which has been lost by the plate.

If a plate of iron be put in this solution, the gold which is precipitated seizes, perhaps equally, a part of the iron, or at least its precipitation is determined by the affinity of the iron for the gold, at the surface of which this last combines. For the gilding is a combination of the two metals at the surface, by which they are in contact: when the first stratum

is formed, the precipitation is capable of continuing by the mere force of cohesion of the gold to itself.

7. What I have just stated is verified in the precipitation of copper by iron. When a solution of copper is decomposed by a plate of iron, and the copper attached to the plate is separated, it is perceived by the brown colour of the interior surface, that the copper is not there pure, and that it contains some iron. After this first stratum the copper continued to precipitate, by contracting an adherence with itself, and afterwards with those coats which are successively formed; so that the affinity for the iron began the effect, which was continued by that of the copper for itself.

8. Phosphorus precipitates several metallic solutions, as has been explained by Sage and Bouillon*. Though it has a strong affinity with oxygen, yet what has just been explained relative to precipitation by the metals, ought to be applied to its action.

Pelletier has proved, that phosphorus has the property of combining with metals, so that a part of that which is put in action is capable of combining with oxygen, while the other acts equally upon the metal.

Among the metallic solutions there are some which are not affected by phosphorus; in others the metal is precipitated in oxide, which doubtless retains part of the acid of the solution, or of the phosphoric acid which is formed; in others the metal is at length reduced. Gold, silver, copper, and mercury, are among those which resume the metallic state.

On considering the observations which have been made on this precipitation, it appears that copper and silver are precipitated by combining with a small portion of phosphorus; thus, to precipitate twelve grains of silver, three grains of phosphorus are consumed. But only about three grains of phosphoric acid, in a gelatinous state, were obtained: now, one grain of phosphorus only would be necessary to produce this quantity of acid; there consequently remain more than two grains, which must have combined with the silver.

A part only of mercury thus treated resumes the metallic state; the rest preserves the state of an oxide, and combines with the phosphoric acid. Here the force of cohesion is weak; and no combination is made with the phosphorus: hence the effect is but partial, as when iron is used. (No. IV.)

Though the affinity of gold for oxygen is very weak, and much inferior to that of copper, its precipitation is not so quickly effected, and a portion is precipitated in the state of oxide; this doubtless happens because it has little disposition to combine with phosphorus; and it is by the combination of copper with phosphorus, that the precipitation of the latter metal is determined.

Some of the observations which I have here presented require the accuracy of experiment for their entire explanation: but all appear to me indubitably to prove, that it is the force of cohesion which tends to recombine the particles of a one individual metal, and it is the

* Journal de Physique, 1781.

mutual affinity of some metals which decides their precipitation in the metallic state; so that this state is more or less sudden and complete, according to the energy with which these causes are capable of acting*.

ARTICLE XIV.

Of the resulting Affinity.

1. I denominate that the *resulting affinity*, whose action proceeds from several affinities in the same substance: for instance, the nitric acid is composed of oxygen and azote; this acid combines with pot-ash; it acts upon pot-ash by an affinity which results from that of the oxygen and that of the azote. The reciprocal mutual action of the pot-ash is likewise a force which results from that exerted by it upon each of the substances which compose the nitric acid.

2. All bodies existing upon the earth have an affinity for each other. If we refuse to admit this principle, it must be allowed, that the number of exceptions can be but very small. I may therefore reason upon this supposition, and apply to all substances that which observation has made known relative to the affinities and their modifications. If this application be not forced, if it accounts for properties which cannot be directly established upon experiment, the considerations which I present in this article may throw some light upon several phenomena, which are owing to a chemical action still undetermined.

3. I have supposed in the definition of *resulting affinity*, that the affinity of a compound substance is derived from those of the substances which compose it. It is necessary to examine, what are the circumstances which might modify the elementary affinities, and ascertain the changes which must have arisen in the affinity resulting from them.

4. The chemical action of substances is weakened in proportion to their saturation. (Article II. No. 10.)

It must be concluded, therefore, that the resulting affinity must be a less quantity than the elementary affinities when alone: for the latter have experienced a commencement of saturation; but other circumstances may increase the action of the resulting affinity, or may augment the weakness it derives from saturation.

5. If one of the substances that combine, change from a solid to a liquid state, it acquires the advantages possessed by solvents; and its affinity, which was disguised by the solid state, becomes active, so that the resulting affinity may, on this account, be much more considerable than the elementary affinities appeared to be.

Thus, when sulphur is dissolved by pot-ash, the sulphuret which proceeds from it exerts a strong action upon oxygen gas, as soon as it has been rendered liquid by the addition of water, or has attracted sufficient humidity from the air: because it has by that means lost

* Fabroni has published some very interesting observations on the mutual action of metals. (Journal de Physique, Brumaire, An. 8.)

its force of cohesion, as if it had been rendered liquid by the action of caloric, and because the pot-ash likewise exerts an action upon the oxygen, though much weaker than that of the sulphur, since it cannot alone overcome the elasticity of the gas. The action of the sulphur is really diminished by the whole of that attraction which it exerts upon the pot-ash, and upon the water which serves as a solvent to the sulphuret; but it gains much more by the liquidity which it acquires, than it loses by this saturation. Strictly speaking, all those substances of which the solidity is surmounted by a solvent, do act by virtue of a resulting force or affinity.

6. Circumstances contrary to the preceding produce an opposite effect; and when substances by combining, become solid, or more disposed to crystallise, this circumstance is added to the loss of force, arising from the saturation.

For example, pot-ash and nitric acid have both the property of dissolving in alcohol, and yet alcohol does not dissolve the nitrate of pot-ash; that is, the force of cohesion which belongs to that combination, and which, with water, produces its crystallisation, has modified the elementary affinities into the resulting affinity. What confirms this explanation is, that salts which are not crystallisable in water, because they oppose only a feeble cohesion, have, in general, the property of dissolving in alcohol; but, in such a manner, that they are capable of crystallising in that fluid, because the weaker action of the alcohol cannot overcome, except to a certain point, the force of cohesion, of which they are not entirely destitute. The limited solubility of nitrate of pot-ash in water, arises from the solubility of the pot-ash, as well as of the nitric acid by water being greater than in alcohol.

7. Bodies act in proportion to their quantity, which exists within the sphere of action. (Art. IV.)

Hence we deduce, that an action much stronger than that of the component parts may result from a combination, when the components, or one of the two, pass from the elastic to the liquid state; for they then carry into the sphere of activity a more considerable quantity, the action of which may considerably exceed the loss of force that is owing to the saturation.

Thus pot-ash cannot overcome the resistance which proceeds from the elasticity of the oxygen gas and the azote gas; but if these two be combined in order to form nitric acid in the liquid state, they act upon the pot-ash in a quantity much greater than when they could have applied in the elastic state; and the result of their action, though weakened by a commencement of saturation, is found to be much more considerable, than if the azote and oxygen had continued in the elastic state:

8. The affinity of a substance which enters into combination with a compound substance, concurs with the elementary affinities of the latter to maintain its composition against the action of foreign substances, in proportion to the degree of saturation which it produces. Thus iron easily carries off oxygen from azote, or rather, it shares it, or takes a portion; but as soon as the nitric acid is combined with pot-ash, iron can no longer separate its
oxygen

oxygen at an ordinary temperature; but at a higher temperature the difference of dilation sufficiently destroys the resulting affinity of the pot-ash to cause the iron to combine with the oxygen.

In the oxygenated muriatic acid, the oxygen which has undergone but little saturation, and which consequently is feebly retained by the muriatic acid, passes easily into other combinations; but though it is found in a much greater proportion in the oxygenated muriate of pot-ash, it is carried off with much more difficulty by oxygenable substances.

The phosphate of lime is not decomposed by charcoal, even at a great degree of heat; but if it be in the state of acidulous phosphate, the portion of acid, which may be considered as in excess at the state of saturation, is capable of being decomposed by charcoal, because it is not prevented by a sufficiently large mass of base; and it is this part alone which affords phosphorus, when, in order to obtain this substance, we use phosphate of lime reduced to an acidulous phosphate by the sulphuric acid.

9. The contrary takes place when, instead of a saturating substance, which serves as a support to the resulting affinity, one is added that tends to form a combination in which one of the constituent parts must enter. For example, when sulphuric acid is added to a mixture of water and iron, this acid favours the decomposition of the water, because it tends to combine with the metal, and with a proportion of oxygen; a tendency concurring with that of the metal against the affinity which forms the combination of oxygen with hydrogen.

10. From the preceding remarks it may be concluded, that the properties of the resulting affinity of compound substances may be reduced, 1. to the advantages of liquidity, and under this point of view it is necessary to apply to it the theory of solvents (Art. IX.); 2. to the disposition to solidity, which produces contrary effects, which must be explained by the force of cohesion (Art. V.); 3. lastly to the concentration of elastic substances; this circumstance demands particular consideration, but it may be found combined with one of the two preceding.

The observations presented in No. 7 and No. 8 prove, that in those compounds in which elastic substances are concentrated, there are established by a change of constitution, affinities which may be considered as new; an additional force has accrued, to which we may apply the inverse of what has been stated relative to the effects of elasticity (Art. VI.)

The distinctive character of the complex affinities, of which I have treated (Art. XII.) compared with those which result from the composition of the substances of which I now speak, is, that in the first but very little change obtains in the constitution of the component parts; so that when the force of cohesion or elasticity does not intervene, we may consider them in the same manner as they have been considered in Article XII, No. 1. whereas a new force is established in compounds in which elastic substances are condensed; a power which may be considered analogous to that of the cohesion which obtains on the mixture of different substances that determine the combinations that are formed, or which require to be overcome by the forces opposed to it.

11. Caloric, by augmenting elasticity, destroys the affinity resulting from substances, of which the constituent parts have an unequal dilation, conformable to what has been stated in Art. VII.

12. Observation farther shews us, that when the resulting affinity is not sufficient to prevent the decomposition, it sometimes renders it very slow and long. It is to this slowness of action, to these progressive changes of constitution, to the different degrees of saturation which take place; that most of the phenomena observable in vegetation, fermentation, the animal economy, and in general among all bodies which contain condensed elastic substances, must be ascribed*. This subject will require still farther explanation.

13. The resulting affinity ought always to be considered as a single force, while the substances from which it is derived remain in combination: but it is necessary to consider the elements of which it is composed when a separation takes place; the latter is then executed conformably to what has been explained in the division of substances, in proportion to the opposite powers which act upon them:

14. It frequently happens that a substance acts partly by a resulting affinity, and partly by its elementary affinities. When a metal is dissolved by the nitric acid, one part of the acid exerts a resulting affinity, and another acts by its elementary affinities, so that the oxygen of the latter part is divided between the metal and the azote, and the oxide which is formed is dissolved in the undecomposed acid.

15. It is evident, from what has just been explained relative to resulting affinity, that a false idea may be adopted of the properties of a body, when we confine ourselves, as is too often done, to the determination of its constituent parts, without paying attention to the other conditions of its constitution, if, among those constituent parts there may be some which have undergone a considerable alteration in their state. A quantity of oxygen gas does not possess the same chemical power when it is in the elastic state, as when it exerts a resulting force in its combination with azote, hydrogen, carbon, sulphur, or a metal.

For example, the oxygen does not exert the same action, and has not the same resulting affinity, in the sulphuric and the sulphureous acid: though in the sulphuric acid a smaller proportion of sulphur is combined, yet it adheres much more strongly than in the sulphureous acid, and being more condensed, it exerts a much more powerful chemical action†. We ought not to confound the oxygen gas which is held in solution by water, with the oxygen which, by its combination with hydrogen, forms this liquid: the difference caused between them by the state of condensation, produces two substances very different in their chemical action.

* I have often had recourse to this change of constitution in the chemical explanations which I have had occasion to give, and particularly in the lectures of the National School, where I defined the resulting affinity by the name of *collective affinity*, and distinguished it from *elementary affinities*.

† I have observed the effects of condensation in a memoir on the sulphureous acid (*Annales de Chimie*, 1789).

It is therefore necessary either to consider all the constitution of a body, in order to explain its chemical properties, or to content ourselves with establishing those properties by experiment; for we must pay attention to all the circumstances of chemical action, to explain the results of that action, or we must confine ourselves to their establishment or confirmation.

ARTICLE XV.

Recapitulation.

1. It has often been remarked that the action of a substance is diminished, in proportion as it approaches to a state of saturation: and this diminution of power was made useful in the explanation of several chemical phenomena. It was affirmed that a metal could only take a definite portion of oxygen from the nitric acid, because when the portion of oxygen in the acid is diminished, the remainder is too strongly combined with the azote. The property of carrying off only a portion of oxygen from certain metallic oxides was attributed to hydrogen; it was admitted that when a substance attracts humidity from the air, its dissolving force comes to an equilibrium; so that according to the degrees of desiccation of the air, it may either carry off or give out water to it. It was known that the resistance found in expelling a substance from the last portions of a combination, either by the action of an affinity, or by that of heat, is much greater than at the commencement of the decomposition, and sometimes such, that its entire decomposition cannot be effected. Thus it was ascertained that oxygen could be but partially disengaged from the oxide of manganese by the action of heat.

The combinations which are formed when forces are opposed, does not therefore depend upon the affinities alone, but upon the proportions of the substances which act. I have therefore only applied to all the phenomena of chemistry, what has been unavoidably admitted in several of them from observation. I have deduced the immediate consequences.

2. These consequences are, that substances act in proportion to their affinity, and their quantity existing within the sphere of activity; that the latter may compensate the force of affinity, and that the chemical action of each power is proportionate to the saturations it produces. I have defined by the word *chemical mass*, or *mass*, the quantities determined by a like degree of saturation, and consequently relative to the capacity of saturation: when two substances are in competition in order to combine with a third, each of them obtains a degree of saturation proportionate to its mass. The subject of the combination also divides its action in proportion to the masses, and by varying the latter the results will also be varied.

3. I have considered all the forces which, by their concurrence or opposition to the mutual affinity of the substances brought in action according to the preceding principle, may have an influence upon chemical combinations and phenomena. They may be reduced to the following heads: the action of solvents, or the affinity which they exert according

to their proportion; the force of cohesion, which is the effect of the mutual affinity of the parts of a substance or combination; the elasticity, whether natural or produced by heat, which ought to be considered as an affinity of caloric; the efflorescence, the cause of which may be attributed to an affinity which is not yet determined, and which acts only under very rare circumstances; gravity likewise exerts its influence, particularly when it produces the compression of elastic fluids; but it may always, without inconvenience, be confounded with the force of cohesion.

4. I have endeavoured to ascertain whether it were possible to determine the relative affinity of two substances for a third; I have observed that for this purpose it would be necessary to discover in what proportions this third would be divided with a given quantity of each of the two first, or rather would divide its action; I have pointed out the insurmountable obstacles that would be met with in the means that must necessarily be employed to prove this division of action, and the changes of constitution which would attend it.

5. As the tables of affinity have all been constructed upon the supposition that substances possess different degrees of affinity, which produce the decompositions and combinations which are formed, independently of the proportions and other conditions which contribute to the results, these tables are calculated only to give a false idea of the degrees of chemical action of the substances arranged in them.

6. The denomination of *elective affinity* is, in itself, erroneous, since it supposes the union of one entire substance with another, in preference to a third, while there is only a division of action, subjected to other chemical conditions.

7. The action of two, three, or a greater number of substances, is subject to the same laws, and the result depends on their affinity, their proportion, or the degree of saturation in which they are found, and the concurrence or opposition of the forces they exert.

In every case of liquidity a mutual saturation takes place, and the result is a single combination, in which all the forces are counterbalanced, while there is neither precipitation nor disengagement of elastic matter; but as the action is divided when there is an opposition of forces and a difference of saturation, some substances are retained in the new combination less strongly than before the mixture; they may consequently yield to the powers of cohesion, elasticity, or other affinities which they might otherwise have resisted.

8. The force of cohesion which has hitherto been considered merely as an obstacle to solution, limits not only the quantities of substances which may be brought into action in a liquid, and consequently modifies the conditions of the saturation which follows; but it is the power which causes the precipitations and crystallisations that take place and determines the proportions of such combinations as are made by quitting the liquid: it is this force which sometimes even produces the separation of a substance, without its forming any combination with another substance, as we have remarked in some metallic precipitations. I have distinguished insolubility from the force of cohesion, because the one is relative only to the action of the solvent, and the other is the effect of the mutual affinity of the parts of a substance or combination, considered absolutely.

Elasticity acts by producing effects opposite to those of cohesion, and which consist either in withdrawing some substances from the action of others in a liquid, or in diminishing the proportion which exists within the sphere of activity; but when all the substances are in the elastic state, their action is subjected to the same conditions.

If tables were formed which would represent to disposition, to insolubility, or volatility, in the different combinations, they would serve to explain a great number of combinations which take their origin from the mixture of different substances, and from the influence of heat.

9. Caloric acts upon bodies like the other solvents, when it is not in a state of *radiant caloric*, because in this case it is not in combination.

It is necessary that it should exceed the greatest part of the force of cohesion, in order to render a body liquid, and other affinities may concur with it to produce this effect, in the same manner as itself concurs with the action of other solvents.

It is not distributed among bodies in proportion to their ponderable quantity, or their bulk, when it produces the degrees of temperature indicated by the thermometer, in the same manner as an acid does not take up an equal quantity of the different alkalis, to attain the same degree of saturation, and the tables of specific caloric are similar to those of specific acidity or alkalinity, which might be constructed: the former determine the capacity of caloric, the latter would determine the capacity of saturation*.

One difference, however, would exist between these tables is, that those of acidity or alkalinity would represent the whole saturation to a certain assumed point, because pure acids and alkalis might be employed: but the specific calorics could only be determined from an unknown point of saturation to another point, because the bodies submitted to experiment are already combined with a quantity of caloric. The results which may be obtained between two degrees of the scale of the thermometer, have no known connection with the total quantities. To attempt to form a conclusion of one from the other, is the same as to pretend to determine the comparative solubility of the muriate of soda and the nitrate of pot-ash in water, by experiments made only at or towards the point of ebullition or of congelation. In the former case we should say that three parts of water would be required for the solution of one part of muriate of soda, and only half a part would be necessary to dissolve one part of the nitrate of pot-ash; in the latter that much less water would be required to dissolve the muriate of soda than to dissolve the nitrate of pot-ash†.

* Several chemists have busied themselves in determining the constituent parts of chemical combinations: no person has carried this point to such an extent, and with so much success, as the celebrated Kirwan; though the methods he employed are subject to some discussion.

The same chemist has established that the affinities are proportionate to the quantities producing the saturation; but he considered them as forces independant of the proportions and other conditions which modify them.

† This consideration alone, that specific caloric has no known relation to the quantity of caloric combined in a body, shews that the experiments by which Rumford has lately pretended to prove that caloric is not a constituent part of bodies, cannot lead to such a conclusion.

On taking the solid state, the force of cohesion in a body obliges a part of the caloric to separate; in the same manner as when a salt crystallises, it abandons part of the solvent, or even a part of the acid, or of the alkali, with which it might be combined.

10. It might be affirmed, that the affinities would be really represented by the tables of capacity; since they would afford the measure of the action of such substance upon another, when a common term of saturation was found, such as neutralisation for the acids and the alkalis, and thermometric temperature for caloric; but nothing could possibly be concluded for chemical action at another term of saturation, and more particularly for another constitution, and for all the circumstances in which the forces of elasticity and cohesion might not be introduced.

11. After having considered all the affinities which may jointly produce chemical action, I examined how, in the compounds, they may result from their constituent parts, in order to acquire a conception how the varied powers which produce all the chemical phenomena, can be derived from one single property of simple bodies.

The observations presented on this subject have shewn, that what principally distinguishes compound substances, whose action is considered as simple is, the condensation of the constituent parts, on which a new affinity depends, an affinity very different from that which the same parts possess in the elastic state: the elementary affinities are modified by the state of saturation, by the force of cohesion, or by the variations of elasticity: the resulting affinity may undergo, by combination, a new degree of saturation which tends to support the composition, or may be weakened by other tendencies to combination with one of the constituent substances.

12. All the considerations which I have presented respecting the modifications of chemical action, do not prevent us from using the term affinity to denote the whole chemical power of a body exerted in a given situation, either by its present constitution, its proportion, or even by the concurrence of other affinities; but we must avoid considering this power as a constant force which produces compositions and decompositions. By so doing we should erroneously infer from its present effects in any instance, what it would be under other conditions which may indeed give it a very different degree of force; by so doing we should neglect all the modifications it may experience from its initial action to that in which it obtains its equilibrium.

13. In this essay I have pointed out an uniform proceeding for the cultivation of chemical science. I have considered all the forces which unite in the combinations and phenomena on which it is employed; I have endeavoured to determine the influence of each in different circumstances. If the reader should regret the loss of all expectation of classing the chemical power of bodies, independently of the conditions which modify it, it will readily be granted, that the tables of affinity contained simple notes of facts nearly sterile, and that they deprived us of the advantage of employing in our deductions the knowledge of the most fruitful properties, by means of which a great number of the results of chemical action may be anticipated; and the facts may be interpreted and directed without having recourse to suppositions, or requiring any particular principles to explain anomalies.

V.

A Memoir on Mortars, Water Lime, Cements, and Pouzzolana. By CIT. GUYTON.

WE find in many publications accounts of processes for the preparations of mortars and cements; enquiries into the composition of those employed by the ancient in edifices still respited by time; experiments to determine the condition of their solidity in the different circumstances; observations on the materials proper to each of the several uses; and descriptions of the characters. It has appeared to me that by collecting and comparing the facts and inferences, we might obtain a small number of truths no longer subject to discussion, which cannot be rendered too familiar to mankind. This is the object of the present memoir, in which I shall also state several experiments hitherto unpublished.

Loriot Mortar.

In 1775, the late M. Loriot directed the mixture of one-fourth part of powdered lime in a good mortar, made as usual of lime and sand, as the essential conditions for obtaining buildings as solid as those of the ancient Romans. See his Memoir, *Journal de Physique*, III. 231.

Having observed that the pulverization of the lime is a painful and even dangerous operation for the workmen, who, notwithstanding every precaution, were subject to bleeding at the nose. I proposed to have the lime covered till it became spontaneously flaked into powder, and then to calcine it again in a small furnace, and use it hot.

This process is explained at full length in a memoir printed in the fourth volume of the *Journal de Physique*, page 481. It has been frequently used in the large way, and always with the most perfect success; which is not to be wondered at, when we consider that two very desirable conditions are thus obtained, namely, that the lime is fresh and in very fine powder.

The description of the oven, with plans, sections, and profiles, is to be found in an instruction printed in 1775, by order of government, and in the *Journal de Physique* for October of the same year, Vol. VI. page 311. The dimensions are sufficient to afford an uninterrupted supply to the labour of a number of workmen, and the proportions of the composition of the mortar, which were thought to be the best, were:

Fine siliceous sand	-	-	-	-	-	o.	3
Cement of well-baked bricks	-	-	-	-	-	o.	3
Slaked lime	-	-	-	-	-	o.	2
Lime spontaneously flaked, and again calcined	-	-	-	-	-	o.	2

10.

Mortar

Mortar of Lafaye.

In 1777, Lafaye published his enquiries into the preparation which the Romans gave to their lime, and the composition of their mortars. The practical part is extracted, with much detail, in the *Journal de Physique*, IX. Part I. p. 437.

The essential article is the method of slaking the lime without liquefying it, in order to have a powder, and still sufficiently caustic to afford a very strong lime, which shall grow hard with age.

For this purpose good fresh lime, made from hard stone, is taken and broken into pieces about the size of an egg; these are thrown into an open basket, which is plunged into water, and kept there till the surface begins to boil; at which period it is taken out, and suffered to drain, when the lime is thrown into casks, where it heats, and falls into powder.

This lime, so prepared, is taken as an ingredient in various kinds of mortar, according to the object. It keeps very well when the casks are covered with straw.

I have used this lime in the construction of a small aqueduct of more than fifty metres in length, intended to convey water to an artificial nitre bed near Dijon, about eighteen years ago. The mortar made of three parts of sand, and three of cuttings of hard lime stone, became very firm and solid in a short time, though it was under ground.

Concerning Poor Lime, or Water Lime (Chaux Maigre).

In some countries they have a kind of lime called poor lime, because in equal quantities it does not give a mortar as fat as the others; but this lime has a property which renders it very valuable, namely, that of becoming solid and hard under water.

The cause of this property was unknown till Bergman observed that it is owing to a oxide of manganese, which the lime stone of Lena contains in the proportion 0.02, as do likewise other stones that form a mortar capable of hardening under water. *Opusc. II. Essay XIX. §. 10.*

In 1783, I published, in the second semestre of the new memoirs of the academy of Dijon, page 90, experiments on six species of this kind of lime stone, among which was the lime stone of Lena, which had been sent me by Bergman. Two among them only assumed by calcination the brown colour which characterises manganese, and underwent comparative essays with the lime stone of Lena.

Independant of the poor lime stone indicated in that memoir, namely, those of Lena, Brion, in the department of Saone et Loire, at the distance of five kilometres from Autun, and of Morex, near Geneva, I am at present acquainted with two others, viz. 1. that employed at Metz, in the works under water, of which the analysis was made in the
course

course of the polytechnic school in the year 6;* and, 2. that which Smeaton, constructor of Eddystone light-house, has described in his account, which was obtained from Abershaw, in the county of Glamorgan. Bibl. Britanique, September, 1796, page 102.

I have no doubt but that stones of the same quality may be found in many other places, and in the territory of the French republic †, that is to say, containing manganese, and naturally mixed with a quantity rather considerable of flint and alumine; for it is evident that this last condition contributes to afford a poor lime stone, or mortar, nearly prepared by nature, and unfit to be mixed with any considerable quantity of sand, or cement, or other dry matter.

Artificial Water Lime (Chaux Maigre).

I have long ago proposed to use, in cases when the natural lime stone is not to be had; a mixture of four parts of grey clay, and six of the black oxide of manganese, with ninety parts of good lime stone reduced to a powder.

This mixture, after having been well calcined and cooled, is worked to the consistence of soft paste, with sixty parts of flint.

In the laboratory of the polytechnic school is preserved a ball of this mortar thus prepared, according to the above proportions, at the sitting of the 29 Pluviose, in the year 5, which was immediately thrown into water, where it hardened, and acquired the specific gravity of 2.231.

A very good water lime may be procured still more cheaply by mixing, with common quick lime, a certain quantity of the white iron ore, which is found plentifully, and is in great part composed of carbonate of lime with manganese, more especially those which are poor in iron.

It is possible, as De Saussure observes in his travels in the Alps, that there may be stones, which though they contain no manganese, may from some other particulars of their composition, nevertheless afford lime capable of hardening under water, such as the ashes Cendree of Tournay, mortar made with the trafs of the Hollanders, &c. But this in no respect weakens the results of observations and experiments on the efficacy of the oxide of manganese; as for instance that of common mortar, which without this addition would immediately become diffused in water, but hardens very speedily when a certain dose of that oxide enters into its composition. Mr. Smeaton here affords us an important remark, namely, that balls made of lime and sand well worked together, and then put into water, were destroyed, whatever were their proportions (Loco cit.)

* In this analysis it was found to contain in the hundred carbonic acid 39.0; lime 44.5; flint 5.25; alumine 1.25; manganese 3.5; oxide iron 3.2; water 2.25; loss 1.05.

† Citizen Lomet found a quarry at Saintrails, in the department of Lot-et-garon, which has been successfully employed in building under water.

Concerning Beton, or Water Cement.

Every composition of mortar intended to hold under water, and capable of becoming almost instantly solid, is called in French Beton.

This water cement may be made in several manners: 1. the mortar Lorient, if well made, holds very well under water. I have used it in the walls of a terrace through which there was not the smallest filtration. I have a mass of this mortar hollowed out into a basin, which when filled with water, and placed on a sheet of paper, does not show the slightest trace of moisture.

The mortar Lafaye hardens less speedily, but when once it has become solid, it holds water very well without being softened.

3. The mortar spoken of by Smeaton, composed of equal parts of lime and trass, or the volcanic ashes of the Hollanders, is a true water cement, as it does not yield at all to that fluid.

4. The compositions of mortar with Pouzzolana may also be used under water, and when once indurated, they surpass all others for the hardness and resistance to the motions of the waters.

The most common water cement is made with the poor lime stone, or water lime. It is formed into prisms in hard paste, which are let down into the water, and form a wall, or otherwise this material is poured between two walls, at the distance of 21 or 22 metres asunder, in the manner of coffer work, for the purpose of forming a suitable defence or inclosure for waters in parts of canals above the surface of the ground.

Methods of supplying the Want of Pouzzolana.

Pouzzolana is a volcanic sand which is brought from the environs of Naples. Though the superiority of mortar formed with this material is well known and determined, yet it is often necessarily dispensed with on account of the expence of carriage, which so greatly enhances the price.

But as we are well acquainted at present with a number of extinct volcanos in France, we may find an abundance of materials to supply the place of the Pouzzolana. The experiment was made in 1787 by Citizen De Cessart, engineer, charged with the construction of the cones of Cherbourg. In the month of August of that year I sent him about 13 kilograms of the extinct volcano of Drevin in the department of Saone-et-Loire.

The basalt was first heated in a small reverberatory furnace, then thrown red-hot into water, and stamped and sifted, so that the largest grain did not exceed the size of a pea. The following are the results of experiments on masonry constructed with this cement of basalt, compared with another cement from the Italian Pouzzolana, according to the letter of Citizen Cessart, dated Feb. 12, 1788.

“ In

“ In order to ascertain perfectly the tenacity of this cement, I caused moulds to be made six inches in length, six in width, and four in thickness, forming small masses of 144 cubic inches.

“ Composition of the cement, 180 cubic inches of basalt, 73 of lime flaked (*fusée à la romaine*) 93 inches of spring water, and 288 stones of granite of the size of a nut, the whole forming a cube of 534 inches.

“ Two moulds of a 144 inches each were filled with this composition. One of these was immersed in the sea for 76 days, the other remained in the air for the same time.

“ When the tenacity of this masonry was to be tried, the extremities of these masses were placed on the edge of a piece of iron: each of them was loaded with a kind of wooden trough, very narrow at bottom, placed across the middle of the mass, and gradually loaded with balls of lead.

“ The same operation was formed with two similar bricks of Italian Pouzzolana, having the same proportions, and having remained likewise 76 days in the sea. It supported 123 pounds before it broke, whereas the piece which remained 76 days in the air, broke under a charge of 71 pounds.”

All these proportions, as the writer observes, are infinitely too small to establish an exact ratio, but it may nevertheless be concluded, that the Pouzzolana basalt of Burgundy is nearly as good as that of Italy, which comes to us by Toulon.

Citizen Cessart in the same letter, demanded six thousand weight of this Pouzzolana to repeat his experiments on a larger scale; to which it appeared, that the difficulties and expence of transportation were at that time an obstacle. But this would no longer be any impediment; as the canal of the Saone and the Loire is in full navigation, and the extinguished volcano of Drevin is no more than one myriameter distant from it.

Another material still more common than basalt, and which may sometimes be a substitute for pouzzolana, is the ashes of pit coal, provided it be a heavy cakeing coal, leaving behind it rather a scoria than a cinder.

I likewise sent this article to Cherbourg in 1787, under the name of light pouzzolana, to distinguish it from that which was made with basalt. It was submitted to the same comparative experiments, and afforded the following comparative results, as is stated in the letter of Citizen Cessart.

“ The bricks which had remained 76 days in the air, supported charges of 115 and 137 pounds before they broke.

“ The same blocks immersed in the sea likewise for 76 days, broke with 48 and 30 pounds, consequently they had lost their consistence in the water of the sea.”

We observe, that mortar made with this ashes is evidently superior to that formed with the pouzzolana of Italy, as its tenacity is almost double. It cannot indeed be employed under water like the cement of water lime, but it is probable that it would succeed very well in works which are constructed dry, and are not to receive water until they have acquired solidity.

With this view I pointed out this preparation for the masonry of the inner faces of the canal of Saone-et-Loire, which were precisely in this condition, and near which there were considerable masses of this matter afforded by the fires of a glass-house.

A D D I T I O N.

Citizen Monge, who visited the ruins of Cæsarea, of which the historian Josephus has given so magnificent a description (Cæsarea Stratonis) at present absolutely without inhabitants, assures me, that he could distinguish in its ruins, by the taste of the architecture and the solidity of the mortar, the different ages of the first construction, and the works successively added after the invasion of the Arabs, and during the Crusades.

“ He observed on the side of the gate at present in ruins, the remains of a temple consecrated to Augustus. Endeavours had been made to support the walls in front by counterforts still in being, which in some places exhibited a concave mould or impression of ornaments of an exquisite taste formed of mortar, which had applied itself against reliefs which are now no longer in being.

The stone of the original structure was corroded by the weather to a great depth, and the mortar projected out beyond it.

He attempted in vain to break off a piece. It was of a very fine and equal texture, apparently composed of fine sand and very little lime well blended together.

I thought that this observation might be received with pleasure at the end of the present memoir.

VI.

On the Combinations of the Metals with Sulphur, by CIT. VAUQUELIN.*

WE may divide the combinations of the metals with sulphur into three orders, namely, first metals and sulphur, which are combinations properly denominated metallic sulphurets, or sulphurated metals. Nature and art present many samples of this kind. 2. Metallic oxides and sulphur; these ought to be called sulphurated metallic oxides, or oxidized metallic sulphurets. They are also very common in nature. 3. Metallic oxides with sulphur and hydrogen. These triple combinations are called metallic hydro-sulphurated oxides.

If the substance which predominates in these different combinations be taken as the base of the nomenclature, we should reverse the order of the common denominations; for in the simple sulphurets the mass of metal always predominates, and so likewise in the sulphurated oxides; but in the hydro-sulphurets at first the metals, then the sulphur, and afterwards the hydrogen predominate.

* *Annales de Chymie*, XXXVII. 57.

So that instead of metallic sulphurets, oxidized metallic sulphurets and metallic hydro-sulphurets, we ought to say sulphurated metals, sulphurated metallic oxides, and sulphuro-hydrogenated metallic oxides.

But custom has predominated in chemistry, either because the preference has been dictated by the facility of pronunciation, or because the terms themselves are less harsh. This however is of no great importance, as it is sufficient that the elements of bodies should be expressed by their names, and the value of these names should be precisely understood.

Concerning the action of the acids on the metallic sulphurets.

Sulphuric acid.

Whenever the sum of the affinities of oxygen for a metallic substance, and of the metallic oxide formed by the sulphuric acid, exceed those of the metal for sulphur, and of oxygen for the sulphureous acid, there is constantly a decomposition of the metallic sulphurets; and the result is a metallic sulphur and sulphurous acid. All these conditions are not constantly necessary, but every one of them concurs in accomplishing the new combinations which are formed, and accelerating the process. There are nevertheless cases, in which for want of one of these conditions no action takes place between the bodies.

The sulphuric acid for example, decomposes the natural sulphurated oxide of iron; or martial pyrites very well, but the muriatic acid cannot decompose it, because the sulphur in this case is not attracted by any body, whereas in the other case it is attracted by the sulphuric acid, which it converts into sulphuric acid; for though I am assured by many facts, that the iron in martial pyrites is oxidized nearly to the point at which it is easily soluble in acids, nevertheless these compounds cannot be decomposed without the formation of a great quantity of sulphurous acid.

This opinion is confirmed by the conditions required for the success of the operation. In fact, the complete decomposition of pyrites is not effected but by means of heat, and a large quantity of concentrated sulphuric acid; for it is evident, that in this case the acid is required not only to saturate the oxide of iron, but also the sulphur. I might also urge in proof of the same opinion, the greater affinity of the muriatic acid for the oxide of iron which it cannot nevertheless separate from the sulphur in those circumstances. It may seem astonishing, that the nitric acid, which has a less affinity with iron, should nevertheless decompose the martial pyrites with so much ease; but we must not deceive ourselves in this respect, by supposing that the acid takes the oxide of iron from the sulphur by a stronger affinity, for it is on the contrary, by giving a new quantity of oxygen to that substance, that its affinity for the sulphur is remarkably diminished. The sulphur does not unite in fact with iron oxidized to the maximum, and if by any peculiar processes these substances become combined, a portion of oxygen is then separated.

Though the muriatic acid cannot decompose the sulphurated oxide of iron, it decomposes on the contrary with the greatest facility the sulphuret of iron, in which the metal is not oxygenated. But in this case three forces act at once, namely, that of the iron on

the oxygen of the water, that of the sulphur on the hydrogen of the same substance, and that of the metallic oxide formed by the muriatic acid. Under these circumstances sulphurated hydrogenous gas is constantly disengaged.

The sulphuric acid which does not decompose the sulphurated oxide of iron, unless it be concentrated and boiled, decomposes the sulphuret of iron in the cold, and the more easily when diluted with a certain quantity of water. The reason of this is evident, and need not be explained.

Though a great quantity of water is inimical to the action of the acids upon metallic sulphurets in general, it is however less so with regard to iron than other metals, because sulphurated hydrogen does not decompose the solutions of iron in acids.

Concerning the Sulphuret of Lead.

In the natural sulphuret of lead or galæna the lead exists in the metallic state, for which reason when this mineral substance is put in contact with muriatic acid, sulphurated hydrogen is formed even in the cold. It appears even when sulphur is united to the oxide of lead. That metal loses its oxygen to unite to the sulphur for the combination by this means obtained, is perfectly similar to natural galena. It is easy to conceive how the reduction of the lead is obtained in this process: a portion of the sulphur combines with the oxygen, and forms sulphuric acid, which is driven off by the heat, while another portion of the sulphur unites to the metallic lead. The reduction of the lead, and the formation of galena take place nearly in the same manner, when a solution of this metal in an acid is mixed with a solution of sulphurated hydrogen, or any hydro-sulphuret whatever. There is however this difference, that in the last case it is hydrogen, which takes oxygen from the lead, and forms water at the same time that the sulphur and the metal, which meet in a very extreme state of division, combine without heat in the midst of the fluid. There is also another difference, that the galena formed by this process contains a little more sulphur than the natural compound, as is proved by the small quantity of sulphur which is separated from that substance by a very gentle heat, and then leaves a compound in a state absolutely similar to that of common galena.

From these facts it results that the muriatic acid, and probably all the acids which act upon the oxide of lead by their mass and their concentration, do lose their lead by the action of the sulphurated hydrogen, as soon as these two forces, or even one of them is weakened; but whatever may be the quantity of acid united to lead, or any other metal, the decomposition of the salt will take place, if any hydro-sulphuret be employed in sufficient quantity.

We cannot be surprized at the diminution of the affinities of the acids for the metallic oxides by water, nor their more powerful action on these substances by their masses. Antimony, mercury, bismuth, and lead itself, afford striking examples of this.

We know in fact, that by mixing a great quantity of water with their concentrated solutions, very considerable precipitates are formed, which have been erroneously taken for pure

pure metallic oxides, but which are really salts, containing a portion of acid too strongly adherent to be separated but by means of an alkali.

We may therefore consider the combinations formed by these metals, and probably by many others likewise with acids as neutral salts, dissolved in a superabundance of acid, which is taken from them by water, when the salts are precipitated in a truly neutral and insoluble state.

From the manner in which the muriatic acid acts upon the metallic sulphurets, it follows, that the more oxygen is absorbed by the metals, in order to unite with this acid, the less of sulphur will remain after the solution, and the contrary; but if these metals absorb but little oxygen, if they contain much sulphur, and if also their affinity for oxygen be not considerable, the whole of the metal will not be separated from the sulphur by the muriatic acid, because in proportion as this combustible body became more abundant, it opposes the solution of the metal by virtue of its mass; this is the reason why certain metallic sulphurets, which are not attacked by the muriatic acid, are decomposed by it, when a portion of their sulphur has been separated in distillation.

The mode of action of the nitric acid upon the metallic sulphurets is very different from this, for though the sulphur is not in this case attracted by any body, it is nevertheless completely separated from the metal; and thrown down in all its purity, if the experiment be properly conducted: but if the combination of the nitric acid with the metal be not aided by hydrogen, that takes place with the muriatic acid, it gives out to the metal a portion of oxygen much more readily than water; and it is in consequence of this affinity of the oxygen for the metal, and of the oxide which is formed for the undecomposed acid, which is effected by the solution. Thus every time that metallic sulphures are decomposed by the nitric acid, there is a constant formation of nitrous gas, and never of sulphurated hydrogen.

It appears that in general sulphur has a greater affinity for the metals than for their oxides, and that this affinity diminishes in proportion to the quantity of oxygen they absorb: they are some which do not even combine with sulphur, except in the complete metallic state; thus for instance, on triturating some oxide of lead with sulphur, a portion of the latter deflagrates, and the other combines with the lead, which is brought to the metallic state.

Hence it appears that zinc does not combine with sulphur when it is completely metallic, at least nature has not hitherto afforded us sufficient examples, and art has not succeeded in effecting the combination of these two substances. What is known in mineralogy under the name of *blende*, or sulphurated zinc, has been improperly considered by some mineralogists as a combination of metallic zinc with sulphur. It is, on the contrary, oxide of sulphurated zinc, which very frequently contains hydrogen; for, the *blendes* almost always dissolve in the acids, and produce sulphurated hydrogen gas. Thus a transparent blend which acts in this manner with the acids, ought to be considered as a real oxide of hidro-sulphurated zinc, or rather sulphureo-hydrogen. There may be oxides of zinc

simply

simply sulphurated, and these will then be two species of minerals very different in their nature; the one dissolves in the sulphuric and muriatic acids without effervescence, and the other, on the contrary, affords a greater or less quantity of sulphurated hydrogen gas.

Mercury, in a metallic state, does not very intimately combine with sulphur; ethiops mineral appears to be rather a mixture than a real combination; but when this oxidized metal is combined with sulphur, as in cinnabar, for example, it adheres so strongly, that no substance is capable of directly decomposing it; and it is only by altering one or the other of these principles that their separation can be effected, or rather it is necessary to burn the sulphur or disoxide the mercury; cinnabar owes its fine red colour only to the great quantity of oxygen which the mercury contains.

VII.

On the Precipitate formed by mixing Muriate of Lime and Muriate of Magnesia. By
W. LAMBE, M. A.

To MR. NICHOLSON.

S I R,

HAVING discovered, what I conceive to be, the true explanation of the appearance of a precipitate by mixing the muriate of lime and magnesia, I take leave to communicate it to you. All the accounts I have met with on this subject are erroneous; and that offered by the celebrated Berthollet is as much so as any (Journal, IV. 67). To refute it, I need only mention a single fact. Let the carbonate of magnesia, before dissolving it in the muriatic acid, be thoroughly deprived of its carbonic acid by heat, and then there will be no precipitation by mixing the two salts. To the carbonic acid, then, is this appearance to be attributed, of which the carbonate of magnesia cannot be wholly deprived by the addition of so much muriatic acid as will produce an apparent saturation.

Dissolve some common magnesia in a solution of carbonic acid in distilled water. To the clear solution add a little muriate of lime, and a precipitate is immediately formed, entirely similar, I believe, to that in question. If the liquor be examined after some days, carbonate of lime is found dissolved in it, instead of carbonate of magnesia. It seems, then, that these precipitates are formed by an interchange of principles between carbonate of magnesia and muriate of lime, and consist of carbonate of lime and muriate of magnesia. The liquor, by evaporation, deposits a crystalline matter, which effervesces with acid, and probably is composed of the same salts in different proportions.

Lastly, put some of the precipitate in the nitric or muriatic acids. It dissolves slowly, with a gradual extrication of air bubbles, and adding acid enough, it may be wholly con-

verted

verted into deliquescent salts; but a good deal of acid is necessary for this purpose. Even distilled vinegar has this effect in part.

I intended to repeat these experiments with greater care and exactness, with a view to the improvement of the analysis of mineral waters; but having been distracted by other occupations, I beg your acceptance of them for the use of your valuable Journal, if you deem them of sufficient consequence. Before I conclude, I will take the opportunity of correcting another error which, I believe, pervades the most recent chemical works. It is this:—Ammonia is said to decompose sulphate of magnesia only in part: but this, also, is owing to the ammonia not having been wholly deprived of carbonic acid. I precipitated the whole of the magnesia from sulphate of magnesia, by using ammoniacal gas in excess. I then filtered the liquor, and boiled it with a fixed alkali; but I only obtained a little lime, discoloured seemingly with oxide of iron.

I am, SIR,

With great respect,

Your obedient servant,

Warwick, May 23, 1801.

W. LAMBE.

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

Gratuitous Analysis of Minerals, by the British Mineralogical Society.

I HAVE received the following address by post, which I give without abridgement or alteration.—W. N.

The immense importance which, at all times, and particularly at present, is attached to improvements in agriculture, and the liberal encouragement to experiments in the art of cultivating the land offered by the patriotic views of the Board of Agriculture, have rendered it a desirable object to assist the inquiries of the farmer in that part of experimental chemistry which determines the nature of soils by an analysis of their several contents.

The British Mineralogical Society, having of late increased the number of their members, find themselves enabled to extend the plan of their institution, from the analysis of *minerals*, in the usual sense of the term, to that of the various soils which are made the basis of agricultural operations.

They therefore give this public notice that they will examine, free of expence, all specimens of earths or soils, with a view of determining the nature and proportion of their different contents, with as much accuracy as shall seem requisite.

They

They require, however, that the persons sending specimens shall comply with the following conditions:

That the specimen be about three or four pounds in weight, inclosed in a deal box properly packed with hay, and along with it an account be added of the parish and county from which it is procured, the name of the sender and his address per post, and an answer to the following queries—What is the depth of the soil?—What the nature of the sub-soil?—is it on a hill or level ground? and, if the former, what is the aspect?—how near to any river or stream?—and is the soil itself dry or springy?—what is the rotation of crops which it usually bears?—has it ever been limed or had any other earth laid on?—Any other particulars worthy of notice may also be added, and a specimen of the sub-soil should all be inclosed.

Before the specimen is packed up, it should be gently dried, either in the sun or in any warm place, for a few days, but must on no account be heated over a fire.

The box containing the specimens must be sent *carriage paid* to Mr. W. H. Pepys, Jun. No. 24, Poultry, London.

The society intend, if consistent with their other occupations, to return the result of the analysis within two months of the arrival of the specimen.

London, April 16, 1801.

Four Essays on Practical Mechanics; the first on Water Wheels; the second on the Steam Engine; the third on Mills; and the fourth on the Simplification of Machinery. By Thomas Fenwick, Coal Viewer. 8vo. p. 84. Newcastle upon Tyne, printed for the Author, and sold by Mawman, London.

THE particular advantage of this treatise is, that it is written with such plainness that the instructions it contains may be carried into effect by those whose acquisitions extend no farther than the first rules of common and decimal arithmetic; in addition to which, as the facts are all deduced from observations and experiments on a large scale of actual work, all descriptions of men may receive advantage from his data respecting mills, steam engines, &c.

D^r Young's Inquiries respecting Sound

Fig. 41.

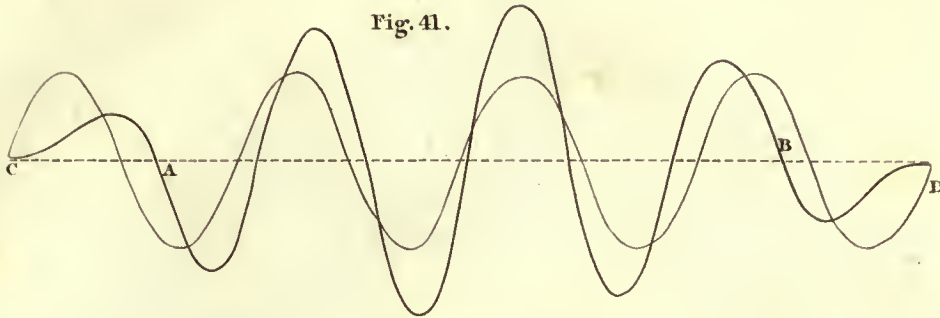


Fig. 42.

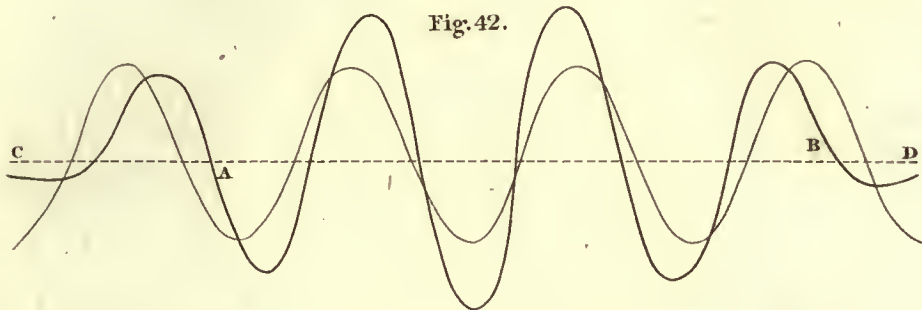


Fig. 44.

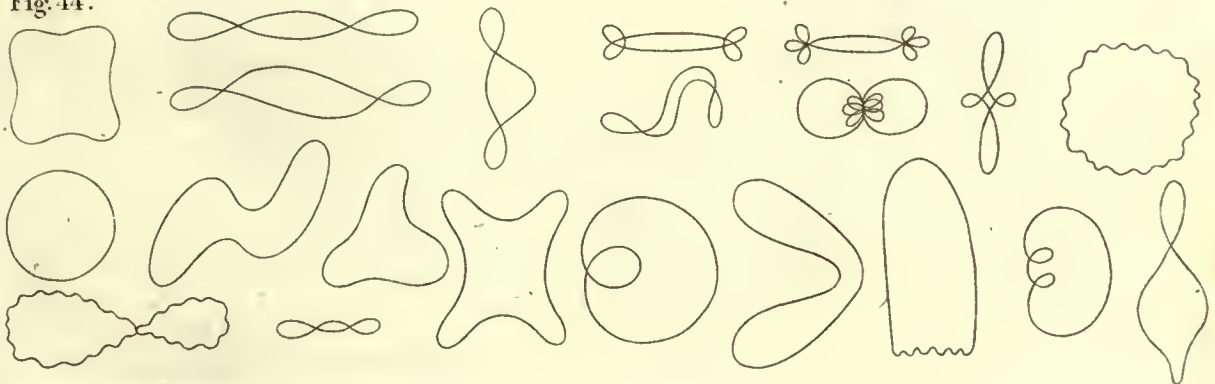


Fig. 45.



Fig. 46.

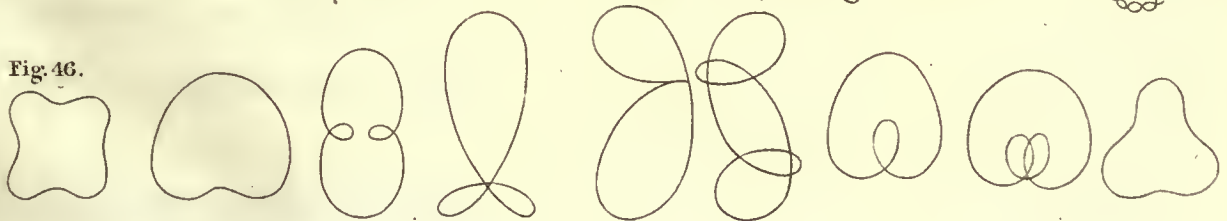


Fig. 49.

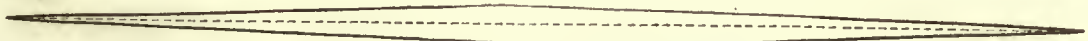
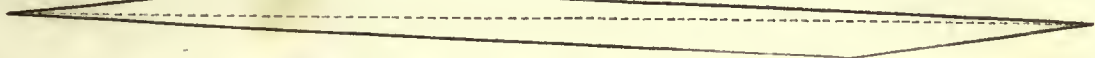


Fig. 50.



1001.9 1001.9 1001.9 1001.9 1001.9



Dr Young's Inquiries respecting Sound.

Fig. 53.

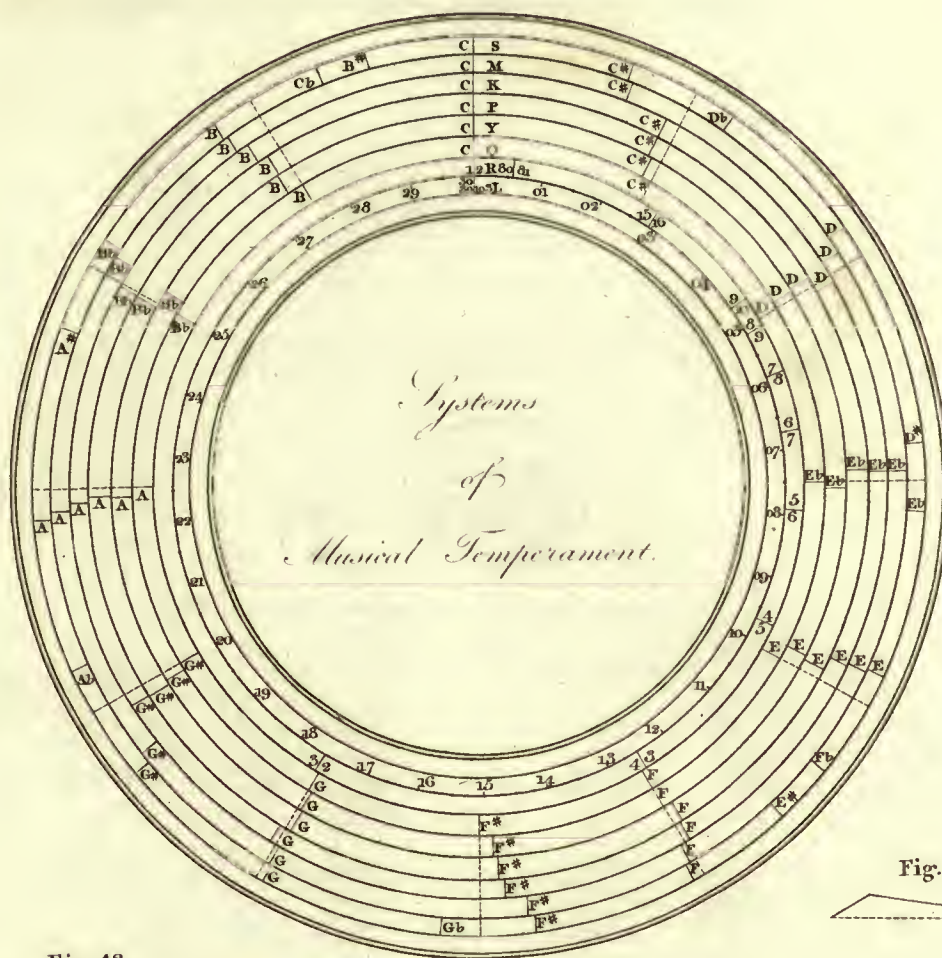


Fig. 43.

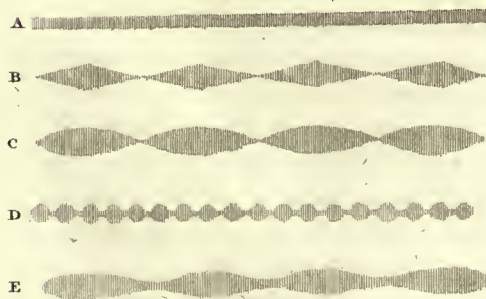


Fig. 52.



Fig. 47.

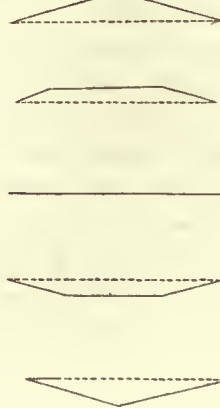


Fig. 51.





PLATE

1. *...*
2. *...*
3. *...*
4. *...*
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6. *...*
7. *...*
8. *...*
9. *...*
10. *...*

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

JULY, 1801.

ARTICLE I.

*Outlines of Experiments and Inquiries respecting Sound and Light. By THOMAS YOUNG,
M.D. F. R. S.*

(Concluded from page 91.)

XIII. *Of the Vibrations of Chords.*

BY a singular oversight in the demonstration of Dr. Brook Taylor, adopted as it has been by a number of later authors, it is asserted, that if a chord be once inflected into any other form than that of the harmonic curve, it will, since those parts which are without this figure are impelled towards it by an excess of force, and those within it by a deficiency, in a very short time arrive at or very near the form of this precise curve. It would be easy to prove, if this reasoning were allowed, that the form of the curve can be no other than that of the axis, since the tending force is continually impelling the chord towards this line. The case is very similar to that of the Newtonian proposition respecting sound. It may be proved, that every impulse is communicated along a tended chord with an uniform velocity; and this velocity is the same which is inferred from Dr. Taylor's theorem; just as that of sound, determined by other methods, coincides with the Newtonian result.

VOL. V.—JULY 1801.

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But, although several late mathematicians have given admirable solutions of all possible cases of the problem, yet it has still been supposed, that the distinctions were too minute to be actually observed; especially, as it might have been added, since the inflexibility of a wire would dispose it, according to the doctrine of elastic rods, to assume the form of the harmonic curve. The theorem of Euler and De la Grange, in the case where the chord is supposed to be at first at rest, is in effect this: continue the figure each way, alternately on different sides of the axis, and in contrary positions; then, from any point of the curve, take an absciss each way, in the same proportion to the length of the chord as any given portion of time bears to the time of one semivibration, and the half sum of the ordinates will be the distance of that point of the chord from the axis, at the expiration of the time given. If the initial figure of the chord be composed of two right lines, as generally happens in musical instruments and experiments, its successive forms will be such as are represented in Plate VI. Figs. 47, 48: and this result is fully confirmed by experiment. Take one of the lowest strings of a square-piano forte, round which a fine silvered wire is wound in a spiral form; contract the light of a window, so that, when the eye is placed in a proper position, the image of the light may appear small, bright, and well defined, on each of the convolutions of the wire. Let the chord be now made to vibrate, and the luminous point will delineate its path, like a burning coal whirled round, and will present to the eye a line of light, which, by the assistance of a microscope, may be very accurately observed. According to the different ways by which the wire is put in motion, the form of this path is no less diversified and amusing, than the multifarious forms of the quiescent lines of vibrating plates, discovered by Professor Chladni, and is indeed in one respect even more interesting, as it appears to be more within the reach of mathematical calculation to determine it; although hitherto, excepting some slight observations of Buffe and Chladni, principally on the motion of rods, nothing has been attempted on the subject. For the present purpose, the motion of the chord may be simplified, by tying a long fine thread to any part of it, and fixing this thread in a direction perpendicular to that of the chord, without drawing it so tight as to increase the tension: by these means, the vibrations are confined nearly to one plane, which scarcely ever happens when the chord vibrates at liberty. If the chord be now inflected in the middle, it will be found, by comparison with an object which marked its quiescent position, to make equal excursions on each side of the axis; and the figure which it apparently occupies will be terminated by two lines, the more luminous as they are nearer the ends, Plate V. Fig. 49. But, if the chord be inflected near one of its extremities, Fig. 50, it will proceed but a very small distance on the opposite side of the axis, and will there form a very bright line, indicating its longer continuance in that place; yet it will return on the former side nearly to the point from whence it was let go, but will be there faintly visible, on account of its short delay. In the middle of the chord, the excursions on each side the axis are always equal; and, beyond the middle, the same circumstances take place as in the half where it was inflected, but on the opposite side of the axis; and this appearance continues unaltered in its proportions, as long as the chord vibrates.

vibrates at all: fully confirming the non-existence of the harmonic curve, and the accuracy of the construction of Euler and De la Grange. At the same time, as M. Bernouilli has justly observed, since every figure may be infinitely approximated, by considering its ordinates as composed of the ordinates of an infinite number of trochoids of different magnitudes, it may be demonstrated, that all these constituent curves would revert to their initial state, in the same time that a similar chord bent into a trochoidal curve would perform a single vibration; and this is in some respects a convenient and compendious method of considering the problem. But, when a chord vibrates freely, it never remains long in motion, without a very evident departure from the plane of the vibration; and, whether from the original obliquity of the impulse, or from an interference with the reflected vibrations of the air, or from the inequability of its own weight or flexibility, or from the immediate resistance of the particles of air in contact with it, it is thrown into a very evident rotatory motion, more or less simple and uniform according to circumstances. Some specimens of the figures of the orbits of chords are exhibited in Plate V. Fig. 44. At the middle of the chord, its orbit has always two equal halves, but seldom at any other point. The curves of Fig. 46, are described by combining together various circular motions, supposed to be performed in aliquot parts of the primitive orbit: and some of them approach nearly to the figures actually observed. When the chord is of unequal thickness, or when it is loosely tended and forcibly inflected, the apses and double points of the orbits have a very evident rotatory motion. The compound rotations seem to demonstrate to the eye the existence of secondary vibrations, and to account for the acute harmonic sounds which generally attend the fundamental sound. There is one fact respecting these secondary notes, which seems intirely to have escaped observation. If a chord be inflected at one-half, one-third, or any other aliquot part of its length, and then suddenly left at liberty, the harmonic note which would be produced by dividing the chord at that point is intirely lost, and is not to be distinguished during any part of the continuance of the sound. This demonstrates, that the secondary notes do not depend upon any interference of the vibrations of the air with each other, nor upon any sympathetic agitation of auditory fibres, nor upon any effect of reflected sound upon the chord, but merely upon its initial figure and motion. If it were supposed that the chord, when inflected into right lines, resolved itself necessarily into a number of secondary vibrations, according to some curves which, when properly combined, would approximate to the figure given, the supposition would indeed in some respects correspond with the phænomenon related; as the coefficients of all the curves supposed to end at the angle of inflection would vanish. But, whether we trace the constituent curves of such a figure through the various stages of their vibrations, or whether we follow the more compendious method of Euler to the same purpose, the figures resulting from this series of vibrations are in fact so simple, that it seems inconceivable how the ear should deduce the complicated idea of a number of heterogeneous vibrations, from a motion of the particles of air which must be

extremely regular, and almost uniform; an uniformity which, when proper precautions are taken, is not contradicted by examining the motion of the chord with the assistance of a powerful magnifier. This difficulty occurred very strongly to Euler; and De la Grange even suspects some fallacy in the experiment, and that a musical ear judges from previous association. But, besides that these sounds are discoverable to an ear destitute of such associations, and, when the sound is produced by two strings in imperfect unison, may be verified by counting the number of their beats, the experiment already related is an undeniable proof that no fallacy of this kind exists. It must be confessed, that nothing fully satisfactory has yet occurred to account for the phenomena; but it is highly probable that the slight increase of tension produced by flexure, which is omitted in the calculations, and the unavoidable inequality of thickness or flexibility of different parts of the same chord, may, by disturbing the isochronism of the subordinate vibrations, cause all that variety of sounds which is so inexplicable without them. For, when the slightest difference is introduced in the periods, there is no difficulty in conceiving how the sounds may be distinguished; and indeed, in some cases, a nice ear will discover a slight imperfection in the tune of harmonic notes: it is also often observed, in tuning an instrument, that some of the single chords produce beating sounds, which undoubtedly arise from their want of perfect uniformity. It may be perceived that any particular harmonic is loudest, when the chord is inflected at about one-third of the corresponding aliquot part from one of the extremities of that part. An observation of Dr. Wallis seems to have passed unnoticed by later writers on harmonics. If the string of a violin be struck in the middle, or at any other aliquot part, it will give either no sound at all, or a very obscure one. This is true, not of inflection; but of the motion communicated by a bow; and may be explained from the circumstance of the successive impulses, reflected from the fixed points at each end, destroying each other: an explanation nearly analogous to some observations of Dr. Matthew Young on the motion of chords. When the bow is applied not exactly at the aliquot point, but very near it, the corresponding harmonic is extremely loud; and the fundamental note, especially in the lowest harmonics, scarcely audible: the chord assumes the appearance, at the aliquot points, of as many lucid lines as correspond to the number of the harmonic, more nearly approaching to each other as the bow approaches more nearly to the point, Plate VI. Fig. 51. According to the various modes of applying the bow, an immense variety of figures of the orbits are produced, Fig. 45, more than enough to account for all the difference of tone in different performers. In observations of this kind, a series of harmonics is frequently heard in drawing the bow across the same part of the chord: these are produced by the bow; they are however not proportionate to the whole length of the bow, but depend on the capability of the portion of the bowstring, intercepted between its end and the chord, of performing its vibrations in times which are aliquot parts of the vibration of the chord: hence it would seem, that the bow takes effect on the chord but at one instant during each fundamental vibration. In these experiments,

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the bow was strung with the second string of a violin : and, in the preparatory application of resin, the longitudinal sound of Chladni was sometimes heard ; but it was observed to differ at least a note in different parts of the string.

XIV. *Of the Vibrations of Rods and Plates.*

Some experiments were made, with the assistance of a most excellent practical musician, on the various notes produced by a glass tube, an iron rod, and a wooden ruler ; and, in a case where the tube was as much at liberty as possible, all the harmonics corresponding to the numbers from 1 to 13, were distinctly observed ; several of them at the same time, and others by means of different blows. This result seems to differ from the calculations of Euler and Count Riccati, confirmed as they are by the repeated experiments of Professor Chladni ; it is not therefore brought forward as sufficiently controverting those calculations, but as showing the necessity of a revision of the experiments. Scarcely any note could ever be heard when a rod was loosely held at its extremity ; nor when it was held in the middle, and struck one-seventh of the length from one end. The very ingenious method of Professor Chladni, of observing the vibrations of plates by strewing fine sand over them, and discovering the quiescent lines by the figures into which it is thrown, has hitherto been little known in this country : his treatise on the phænomena is so complete, that no other experiments of the kind were thought necessary. Glass vessels of various descriptions, whether made to sound by percussion or friction, were found to be almost intirely free from harmonic notes ; and this observation coincides with the experiments of Chladni.

XV. *Of the Human Voice.*

The human voice, which was the object originally proposed to be illustrated by these researches, is of so complicated a nature, and so imperfectly understood, that it can be on this occasion but superficially considered. No person, unless we except M. Ferrein, has published any thing very important on the subject of the formation of the voice, before or since Dodart ; his reasoning has fully shown the analogy between the voice and the *voix humaine* and regal organ-pipes : but his comparison with the whistle is unfortunate ; nor is he more happy in his account of the falsetto. A kind of experimental analysis of the voice may be thus exhibited. By drawing in the breath, and at the same time properly contracting the larynx, a slow vibration of the ligaments of the glottis may be produced, making a distinct clicking sound : upon increasing the tension, and the velocity of the breath, this clicking is lost, and the sound becomes continuous, but of an extremely grave pitch : it may, by a good ear, be distinguished two octaves below the lowest A of a common bass voice, consisting in that case of about 26 vibrations in a second. The same sound may be raised nearly to the pitch of the common voice ; but it is never smooth and clear, except perhaps in some of those persons called ventriloquists. When the pitch is raised still higher, the upper orifice of the larynx, formed by the fummits of the arytenoid cartilages.

cartilages and the epiglottis, seems to succeed to the office of the ligaments of the glottis, and to produce a retrograde falsetto, which is capable of a very great degree of acuteness. The same difference probably takes place between the natural voice and the common falsetto: the rimula glottidis being too long to admit of a sufficient degree of tension for very acute sounds, the upper orifice of the larynx supplies its place; hence, taking a note within the compass of either voice, it may be held, with the same expanse of air, two or three times as long in a falsetto as in a natural voice; hence, too, the difficulty of passing smoothly from the one voice to the other. It has been remarked, that the larynx is always elevated when the sound is acute: but this elevation is only necessary in rapid transitions, as in a shake; and then probably because, by the contraction of the capacity of the trachea, an increase of the pressure of the breath can be more rapidly effected this way, than by the action of the abdominal muscles alone. The reflection of the sound thus produced from the various parts of the cavity of the mouth and nostrils, mixing at various intervals with the portions of the vibrations directly proceeding from the larynx, must, according to the temporary form of the parts, variously affect the laws of the motion of the air in each vibration, or according to Euler's expression, the equation of the curve conceived to correspond with this motion, and thus produce the various characters of the vowels and semi-vowels. The principal sounding board seems to be the bony palate: the nose, except in nasal letters, affords but little resonance; for the nasal passage may be closed, by applying the finger to the soft palate, without much altering the sound of vowels not nasal. A good ear may distinctly observe, especially in a loud bass voice, besides the fundamental note, at least four harmonic sounds, in the order of the natural numbers; and, the more reedy the tone of the voice, the more easily they are heard. Faint as they are, their origin is by no means easy to be explained. This observation is precisely confirmed, in a late dissertation of M. Knecht, published in the musical newspaper of Leipzig. Perhaps, by a close attention to the harmonics entering into the constitutions of various sounds, more may be done in their analysis than could otherwise be expected.

XVI. *Of the Temperament of Musical Intervals.*

It would have been extremely convenient for practical musicians, and would have saved many warm controversies among theoretical ones, if three times the ratio of 4 to 5, or four times that of 5 to 6, had been equal to the ratio of 1 to 2. As it happens to be otherwise, it has been much disputed in what intervals the imperfection should be placed. The Aristoxenians and Pythagoreans were in some sense the beginners of the controversy. Sauveur has given very comprehensive tables of a great number of systems of temperament; and his own now ranks among the many that are rejected. Dr. Smith has written a large and obscure volume, which, for every purpose but for the use of an impracticable instrument, leaves the whole subject precisely where it found it. Kirnberger, Marpurg, and other German writers, have disputed with great bitterness, almost every one for a particular method of tuning. It is not with any confidence of success, that one more attempt is

made,

made, which rests its chief claim to preference, on the similitude of its theory to the actual practice of the best instrument-makers. However we estimate the degree of imperfection of two tempered concords of the same nature, it will appear, that the manner of dividing the temperament between them does not materially alter its aggregate sum; for instance, the imperfection of a comma in a major-third, occasions it to beat very nearly twice as fast as that of half a comma. If indeed the imperfection were great, it might affect an interval so materially as to destroy its character; as, in some methods of temperament, a minor third diminished by two commas approaches more nearly to the ratio 6:7, than to 5:6; but, with this limitation, the sum of harmony is nearly equal in all systems. Hence, if every one of the twelve major and minor thirds occurred equally often in the compositions which are to be performed on an instrument, it would be of no great consequence, to the sum of the imperfections, among which of the thirds they were divided: and, even in this case, the opinion of the best practical authors is, that the difference of character produced by a difference of proportions in various keys, would be of considerable advantage in the general effect of modulation. But, when it is considered, that upon an average of all the music ever composed, some particular keys occur at least twice as often as others; there seems to be a very strong additional reason for making the harmony the most perfect in those keys which are the most frequently used; since the aggregate sum of all the imperfections which occur in playing, must by this means be diminished in the greatest possible degree, and the diversity of character at the same time preserved. Indeed, in practice, this method, under different modifications, has been almost universal; for, although many have pretended to an equal temperament, yet the methods which they have employed to attain it have been evidently defective. It appears to me, that every purpose may be answered, by making C: E too sharp by a quarter of a comma, which will not offend the nicest ear; E: G*, and A^b: C, equal; F*: A* too sharp by a comma; and the major thirds of all the intermediate keys more or less perfect, as they approach more or less to C in the order of modulation. The fifths are perfect enough in every system. The results of this method are shown in Table XII. In practice, nearly the same effect may be very simply produced, by tuning from C to F, B^b, E^b, G*, C*, F* six perfect fourths; and C, G, D, A, E, B, F*, six equally imperfect fifths, Plate VI. Fig. 52. If the unavoidable imperfections of the fourths be such as to incline them to sharpness, the temperament will approach more nearly to equality; which is preferable to an inaccuracy on the other side. An easy method of comparing different systems of temperament is exhibited in Plate VI. Fig. 53, which may easily be extended to all the systems that have ever been invented.

Table XII.

A		B	C
C	50000	1 C + .0013487	1 A, E — .0023603
B	53224	2 G, F .0019006	2 D, B .0029122
B ^b	56131	3 D, B ^b .0024525	3 G, F* .0034641
A	59676	4 A, E ^b .0034641	4 C, C* .0044756
G*	63148	5 E, A ^b .0044756	5 F, G* .0049353
G	66822	6 B, C* .0049353	6 B ^b , E ^b .0053950
F*	71041	7 F* .0053950	
F	74921		
E	79752	D	
E ^b	83810		
D	89304	1 E ^b , G*, C*, F* — .0000000	
C*	94723	2 F, B ^b , E, B .0004597	
C	100000	3 C, G, D, A .0011562	

A, shows the division of a monochord corresponding to each note, in the system proposed. B, the logarithm of the temperament of each of the major thirds. C, of the minor thirds. D, of the fifths; C and D being both negative.

Thus, Sir, I have endeavoured to advance a few steps only, in the investigation of some very obscure but interesting subjects. As far as I know, most of these observations are new; but, if they should be found to have been already made by any other person, their repetition in a connected chain of inference may still be excusable. I am persuaded also, that at least some of the positions maintained are incontrovertibly consistent with truth and nature; but, should further experiments tend to confute any opinions that I have suggested, I shall relinquish them with as much readiness as I have long since abandoned the hypothesis which I once took the liberty of submitting to the Royal Society, on the functions of the crystalline lens.

I am, &c.

Emanuel College, Cambridge,
8th July, 1799.

THOMAS YOUNG.

EXPLANATION OF THE FIGURES.

(See Plates IV. V. and VI.)

Plate IV.

Figs. 1—6. The section of a stream of air from a tube .07 inch in diameter, as ascertained by measuring the breadth of the impression on the surface of a liquid. The pressure impelling the current, was in Fig. 1, 1 inch. Fig. 2, 2. Fig. 3, 3. Fig. 4, 4. Fig. 5, 7. Fig. 6, 10.

Figs.

Figs. 7—12. A similar section, where the tube was .1 in diameter, compared with the section as inferred from the experiments with two gages, which is represented by a dotted line. From this comparison it appears, that where the velocity of the current was small, its central parts only displaced the liquid; and that, where it was great, it displaced, on meeting with resistance, a surface somewhat greater than its own section. The pressure was in Fig. 7, 1. Fig. 8, 2. Fig. 9, 3. Fig. 10, 4. Fig. 11, 7. Fig. 12, 10.

Figs. 13—20. A, the half section of a stream of air from a tube .1 in diameter, as inferred from experiments with two water gages. The pressure was in Fig. 13, .1. Fig. 14, .2. Fig. 15, .5. Fig. 16, 1. Fig. 17, 3. Fig. 18, 5. Fig. 19, 7. Fig. 20, 10. The fine lines, marked B, show the result of the observations with an aperture .15 in diameter opposed to the stream; C with .3; and D with .5.

Figs. 21—23. A, the half section of a current from a tube .3 in diameter, with a pressure of .5, of 1, and of 3. B shows the course of a portion next the axis of the current, equal in diameter to those represented by the last figures.

Fig. 24. The appearance of a stream of smoke, forced very gently from a fine tube. Fig. 25 and 26, the same appearance when the pressure is gradually increased.

Fig. 27. See Section III.

Fig. 28. The perpendicular lines over each division of the horizontal line show, by their length and distance from that line, the extent of pressure capable of producing, from the respective pipes, the harmonic notes indicated by the figures opposite the beginning of each, according to the scale of 22 inches parallel to them. The larger numbers, opposite the middle of each of these lines, show the number of vibrations of the corresponding sound in a second.

Figs. 29—33. See Section X. *

Fig. 34. The combination of two equal sounds constituting the interval of an octave, supposing the progress and regress of the particles of air equable. Figs. 35, 36, 37, a similar representation of a major third, major tone, and minor sixth.

Fig. 38. A fourth, tempered about two commas.

Fig. 39. A vibration of a similar nature, combined with subordinate vibrations of the same kind in the ratios of 3, 5, and 7.

Fig. 40. A vibration represented by a curve of which the ordinates are the sines of circular arcs increasing uniformly, corresponding with the motion of a cycloidal pendulum, combined with similar subordinate vibrations in the ratios of 3, 5, and 7.

* By some oversight the figures 29 to 40 inclusive are not inserted in the Engravings. They shall appear in our next.—N.

Plate V.

Figs. 41 and 42. Two different positions of a major third, composed of similar vibrations, as represented by figures of lines.

Fig. 43. A contracted representation of a series of vibrations. A, a simple uniform sound. B, the beating of two equal sounds nearly in unison, as derived from rectilinear figures. C, the beats of two equal sounds, derived from figures of lines. D, a musical consonance, making by its frequent beats a fundamental harmonic. E, the imperfect beats of two unequal sounds.

Fig. 44. Various forms of the orbit of a musical chord, when inflected, and when struck.

Fig. 45. Forms of the orbit, when the sound is produced by means of a bow.

Fig. 46. Epitrochoidal curves, formed by combining a simple rotation or vibration with other subordinate rotations or vibrations.

Figs. 47 and 48. (in Plate VI.) The successive forms of a tended chord, when inflected and let go, according to the construction of De la Grange and Euler.

Figs. 49. The appearance of a vibrating chord which had been inflected in the middle, the strongest lines representing the most luminous parts.

Fig. 50. The appearance of a vibrating chord, when inflected at any other point than the middle.

Plate VI.

Fig. 51. The appearance of a chord, when put in motion by a bow applied nearly at one third of the length from its end.

Fig. 52. The method of tuning recommended for common use.

Fig. 53. A comparative view of different systems of temperament. The whole circumference represents an octave. The inner circle L is divided into 30103 parts, corresponding with the logarithmical parts of an octave. The next circle R shows the magnitude of the simplest musical and other ratios. Q is divided into twelve equal parts, representing the semitones of the equal temperament described by Zarlino, differing but little from the system of Aristoxenus, and warmly recommended by Marpurg and other late writers. Y exhibits the system proposed in this paper as the most desirable; and P the practical method nearly approaching to it, which corresponds with the eleventh method in Marpurg's enumeration, except that, by beginning with C instead of B, the practical effect of the temperament is precisely inverted. K is the system of Kirnberger and Sulzer; which is derived from one perfect third, ten perfect and two equally imperfect fifths. M is the system of mean tones, the *sistema partecipato* of the old Italian writers, still frequently used in tuning organs, approved also by Dr. Smith for common use. S shows the result of all the calculations in Dr. Smith's harmonics, the system proposed for his changeable harp-chord, but neither in that nor any other form capable of practical application.

II.

On the Means of foretelling the Character of the Summer Season, and the Benefits to be expected from the Cultivation of Grasses which vegetate at low Temperatures. In a Letter from Dr. THO. BEDDOES.

TO MR. NICHOLSON.

SIR,

MANY years ago it occurred to me, that the general nature of our summers might be anticipated in the spring, and sometimes in winter. I have frequently hazarded a prediction, and I begin to acquire some confidence in my rule.

It is founded on the effect of melting substances, particularly snow, upon temperature. In applying this fact to meteorology, I say, *if there be an unusual fall of snow in the countries to the N. and N. E. our summer, cæteris paribus, will be cold and wet.* In the winter of 1798 and 1799, I observed in the public papers frequent mention of immense falls of snow in these hyperborean regions; and you know what our summer was. From the best information I could procure, I collected the contrary during the two last winters; and I prophesied accordingly.

I do not take the word *summer* in the strict astronomical sense, otherwise it would be too soon to talk of that of 1801; and still the approaching season may seem to put my system to shame. But observe, Sir, that I do not pretend to be acquainted with all the different causes of atmospherical refrigeration; and I only point out one, the most decisive perhaps that exists, but still capable of counteraction, and for which there may be a substitute when it is not present.

Some of your readers may perhaps think of trying what I advance by a reference to meteorological tables. But you will remark, that they do not supply the requisite data. This is at least the case with those to which it is in my power to have access; and I wish any hint from me may occasion a fact so essential to us, to be annually ascertained as far as it can be done with accuracy. The snow that may fall in England is of small account; though I suppose an unusual fall here may often accompany an unusual fall in the countries from Jutland to Tobolsk.

I shall now trouble you with another winter speculation, that may lead to experiments in future beneficial. In mild winters there can, I suppose, be no doubt of the vegetation of the grasses. It has often appeared to me, that it goes on unequally in different grasses; and I believe I could name some which grow at a temperature not greatly exceeding 40°. However, I shall content myself with proposing it to persons of more leisure, to ascertain by actual weight and measure, *what is the constitution of the different individuals of this family*

with regard to temperature. If certain grasses of good quality should grow at a temperature lower than that at which others will grow, it would evidently be desirable to have winter pastures and water-meadows of such grasses.

It would likewise be desirable to extend these experiments to our highest summer heat, with due attention to moisture and light.

I am, SIR,

Your's respectfully,

THOMAS BEDDOES.

Clifton, June 9, 1801.

III.

Analysis of Manachanite from Botany Bay. By RICHARD CHEVENIX, Esq. F.R.S. and M.R.I.A. Communicated by the Author.

ABOUT three months ago, I received from the Right Honourable President of the Royal Society, a quantity of sand, which had been found on the sea coast in Providence Island, and thence brought over to England in the course of the winter. It may not be very easy to determine the mineralogical character of any substance in the state of fine powder; but, from its general appearance, the object of our present inquiry was judged to be Manachanite. It was in fine grains, almost as small as sea sand; it was mixed with many white, and some red globules of the same size with the black; and, in this alone, differed from the manachanite of Cornwall. The white globules seemed to be quartz; and the red, as far as could be judged, appeared to be garnet, more than ruby. The specific gravity of so fine a sand, I did not think a character more certain than any other; therefore did not give myself much trouble to take it with accuracy. From the rough experiment I made, it did not appear to be very different from that of the known manachanites. It was slightly magnetic also; but not satisfied with any external marks, I proceeded to try what information I could receive from chymical analysis.

100 parts of the above sand from Providence Island, were exposed to a red heat for half an hour, and weighed upon cooling. No sensible augmentation or diminution of weight had taken place.

Sulphuric, nitric, muriatic, and nitro-muriatic acids, were heated for twelve hours upon a given quantity; and, upon trial with various re-agents, exhibited no marks of having operated an effectual solution.

1. I then took one hundred grains, and treated them with pot-ash, in a platina crucible, in the usual manner of treating hard stones. In about two minutes after the pot-ash was in fusion,

fusion, the mass became brownish, and quickly changed to a rich emerald green. After keeping it red hot for half an hour, the crucible was withdrawn from the fire, and the mass diluted in water. The liquor, not without sediment, was of a very beautiful green, as is usual in fossils, where iron is abundant, and but little oxidated.

2. Muriatic acid was then poured in, and a total solution ensued. This liquor was set on a sand heat to evaporate at a moderately elevated temperature. After some time, a white matter, much like curdled milk, began to appear, and afterward increased to a considerable volume. This substance, separated by filtration, was well washed, and the liquor again exposed to the same temperature was evaporated to dryness.

3. Upon pouring in a large quantity of distilled water to re-dissolve the mass, a precipitate, which was silica, remained behind; but the liquor passed very turbid through the filtre, although repeated attempts were made to render it limpid by that method.

4. A solution of pot-ash was poured in excess into this turbid liquor, a red and considerable precipitate fell to the bottom. The whole was made to boil; but no diminution took place in the apparent volume of the precipitate. The liquor was filtered, and essayed for alumina; but the pot-ash did not appear to have dissolved any thing.

5. The red precipitate was again dissolved in muriatic acid, and essayed by the known re-agents, for the different earths and metals. From these trials it appeared to be oxide of iron, mixed with a small portion of the same substance as that which had coagulated in the first attempt to evaporate the intire solution, as above stated.

6. I now proceeded to examine the nature of the white substance (Exp. 2d.) which had separated itself by spontaneous precipitation. I attempted to re-dissolve a portion of it in muriatic acid, but could not succeed. Nitric acid was much more effectual; but sulphuric acid, after being gently heated with it, operated a perfect solution. Some trials, made with the different re-agents upon the latter solution, afforded the following results:

With pot-ash, soda, and ammonia, a white precipitate; with carbonates of pot-ash, of soda, and of ammonia, ditto; with a muriate or a nitrate, no precipitate; with an oxalate, a white precipitate; with a tartrate, a white precipitate; with a phosphate, a copious white precipitate; with an arseniate, a ditto ditto; with sulphurated hydrogen water, no precipitate; with an alkaline hydro-sulphuret, a dirty glass green precipitate; with a prussiate, a dark blueish green, which shewed that there was still some iron; with tincture of galls, a blood red, and very thick precipitate; with phosphoric acid, a white precipitate; with arsenic acid, a white precipitate.

These experiments, and particularly the last three, were sufficiently characteristic, and proved that this substance could be no other than oxide of titanium. However, it was still contaminated with iron; and a part of it appeared to have remained with that red precipitate, which was already mentioned. To effect a complete separation was still a desideratum. For this purpose, I repeated the experiments upon different portions of the sand. After fusion with pot-ash, and solution in muriatic acid, I attempted to precipitate the whole, and then to dissolve in an excess of carbonate of ammonia, the oxide of iron, become

red by the various treatments. But although red oxide of iron is easily soluble in this salt, it is not sufficiently so to answer the purpose, when oxide of titanium is present. Such instances of the inefficacy of methods, as analytic means, although the primitive facts upon which they are founded are rigidly true, are frequent in chymistry; and indeed it may almost be said, that no two substances come in contact, without producing a reciprocal change in their properties. Hence we must be contented to make slow progress in the art of analysis; and perhaps there may be an inherent principle in the chymical nature of all bodies, that will long impede our way to absolute precision. I was therefore obliged to renounce this method.

I had perceived that phosphoric and arsenic acids formed a precipitate in fulphate of titanium, while neither of these acids produced any alteration in fulphate of iron. I therefore precipitated, by phosphoric acid, an intire solution of the sand in sulphuric acid, and by that means operated as good a separation of the oxides of iron, and of titanium, as is generally obtained in chymical processes. But we are ignorant of the quantity of titanium contained in salts of this metal, or in its oxide; for as yet it has never been obtained in sufficient abundance, reduced to the metallic state; and we have not been able to determine the proportion of oxygen that it may take up in its different stages of oxidation. But I had found about eleven of silica, and forty-nine of black oxide of iron. The remainder must therefore be oxide of titanium, and the proportions in the quintal will stand thus:

Silica	-	-	-	-	11
Oxide of iron	-	-	-	-	49
Oxide of titanium	-	-	-	-	40

100

proportions which differ but little from the Cornish manachanite. Some allowance, however, should be made for the inevitable loss.

The degree of fire necessary to operate a perfect reduction of titanium, exceeds what we have been able to produce. At least neither Mr. Klaproth nor Mr. Vauquelin appears to have succeeded in obtaining any large quantity.

I have never been so fortunate as to obtain this metal reduced and pure, except in small spicula, disseminated through the flux I had used; therefore not in a state fit to be subjected to experiment; but I have obtained it, combined with another substance, and without the aid of any alloy. I mixed with charcoal some phosphate of titanium, and, having added a flux of borax, put it into a double Hessian crucible well luted, and exposed the whole to a violent heat in a forge, into which the wind was impelled through four holes, and by two pair of bellows heavily loaded. After giving at first a gentle heat, increasing it by degrees for three quarters of an hour, and then suddenly raising it as high as I could raise it by the above apparatus, and maintaining at the same degree for half an hour, the crucible was withdrawn and broken. The upper crucible, which served as cover to the whole, was lined with a shining metallic coat, and a metallic button was found in the bottom.

bottom. This button was phosphuret of titanium; it was of a pale white, not devoid of lustre. It was easily broken, and its fracture was grainy and white. It did not melt before the blow pipe, and seemed not very fusible. Muriatic and sulphuric acids did not attack it, nor did nitric acid very easily when cool; nitro-muriatic acid had much more effect upon it, and the liquor gave traces of titanium, and of phosphoric acid.

Phosphoric and arsenic acids were observed by Mr. Klaproth, when he discovered this metal, to precipitate oxide of titanium from the other acids, by direct affinity. However, an excess of muriatic acid can dissolve phosphate and arseniate of titanium; but sulphuric, or any other acid I have tried, has not that property.

In order to obtain oxide of titanium totally free from every other metal, with which it is accompanied in nature, such as iron or manganese, either of the above acids may be used, and that precipitate treated with fixed alkalis, or their carbonates.

Mr. Vauquelin has given a very ingenious explanation of the reason, for which nitric and muriatic acids when heated, cannot retain the oxide of titanium in solution. With the former, it is because the oxide takes a new portion of oxygen from the nitric acid, and becomes too much oxidated to remain in solution. With the latter the reverse is the case, and oxygenated muriatic acid is disengaged. However, in evaporating a large portion of the solution of this sand, which had been effected by the means of pot-ash, and muriatic acid, as above stated, I obtained at the end a salt well crystallized, which, upon strict examination, exhibited all the properties of oxygenated muriate of pot-ash. Hence it is evident that the oxygen had passed, probably in its nascent state, into the muriate of pot-ash already formed; for the first portion of muriatic acid, and long before a total solution of the sand could have taken place, must, by the laws of affinity, have gone to saturate the pot-ash employed. I have attempted to produce a similar combination by the means of manganese, but without success. The circumstances are, not indeed rigidly similar; for in one case we effect a solution, and in the other, a precipitation of the metal that gives out the oxygen, and that may produce some modification.

If the mineralogical riches of the country, from which this sand was brought, are at all to be prejudged, from the valuable productions with which we have become acquainted in the vegetable and animal kingdoms, we may look forward to many interesting discoveries; and already, a metal is found to be a native of those climates, which has not long been known, and has been but rarely met in Europe.

IV.

A Series of Experiments upon Metals with an Electrical Battery, shewing their Property of absorbing the Oxygen from the Atmosphere when exploded by Electric Discharges. By JOHN CUTHBERTSON, Philosophical Instrument Maker, No. 54, Poland Street, London. Communicated by the Author.

THE discovery that metals could be fused by electric discharges, gave rise to a supposition that they might also be converted into oxides of the same means. Many attempts have been made to ascertain this, but the fact has hitherto remained without proof. In the year 1787, Dr. Van Marum and myself produced floculi from different metals by subjecting them to strong electric discharges, and also formed beautiful figures upon paper by exploding metallic wires extended at a small distance from its surface. We imagined that the floculi, as well as the coloured figures upon the paper, were oxides of the metals we made use of; and in order to prove this, we entered upon a course of experiments. But having, perhaps from improper management, the misfortune to break several glasses in the process, Dr. Van Marum declared himself so much discouraged by these accidents, as to decline prosecuting the subject.* When we consider, however, the opulence of the society of which he was director, it is not easy to conceive that so trifling an accident as the fracture of a few glasses should be a sufficient reason for his relinquishing this investigation, more especially as he had the command of an electrical apparatus which I made for that society, not only the most proper for that purpose, but unequalled in the whole world, and from which I now fear we have little to expect.

In 1793—1794, I undertook two different courses of experiments at Amsterdam upon the same subject; but both ended unsatisfactorily, and afforded very little more information than we before possessed.†

On my return to London, after delivering a course of lectures upon electricity, I once more returned to the above-mentioned subject. My previous experiments had taught me that it was necessary to have an apparatus into which atmospherical air or gas could be introduced, and any desirable quantity of metallic wire, so that it would be exploded at pleasure, and the diminution of the air or gas accurately measured. I invented the following apparatus, which possessed all the requisites.

* Second Continuation des Experiences faites par la Moyen de la Machine Electrique Teglerienne par Martinus van Marum à Haarlem, page 272.

† Eigenschappen der Electriciteit 3de, deel blad. 2, 143. Amsterdam, 1794, door John Cuthbertson.

Description of an Apparatus in which Metallic Wires are converted into Oxides by Electric Discharges.

Fig. 11, Plate VII. *a b, c d* is a glass receiver, about ten inches high, and six inches diameter. To the widest end is cemented a large round brass plate, *a b*, perfectly air tight, the other end is mounted with a brass collar, upon which a brass cap, *p q*, is screwed, also perfectly air tight. *f* is an air cock, one end of which is screwed into the middle of *p q*, and the other into a foot on which the instrument stands; *n o* are two air cocks screwed fast to the broad brass plate, with holes communicating with the inside of the receiver. Upon each of these cocks is screwed a glass tube bent, as is represented; their communication with the inside of the receiver is opened or shut by means of *u* and *o*. In the inside of *p q* is a wheel and axis moving upon two pivots, serving to wind up a quantity of wire in readiness for exploding. *m* is a brass tube about three inches long, and three quarters of an inch in diameter, stopped at each end with cork, and the middle filled with hogs'-lard; this serves to draw the wire through ready for exploding.

The wire which is intended for use, must be bound to a packthread of the same length, at different places, and at about four inches asunder; it must be wound upon the axis of the wheel in *p q*; the tube *m* must be screwed off from the plate, and the end of the wire must be directed through the hole wherein *m* was, by means of a long brass needle, which must be pushed through the cork and hogs'-lard, in *m*, with the packthread and wire in the eye of the needle; when it is drawn through, the packthread and wire will follow, and be kept air tight by the hogs'-lard and cork; *m* must be screwed to the plate as before, and the packthread drawn through *m* till it is seen nearly straight in the inside of the receiver, as at *b*. In this state, pour into the lower bend of *k* about half of an inch of mercury, and in the lower bend of *i* a little water, represented by the dots in each. It is evident, from the construction of the instrument, that if all the screws be perfectly air tight, the cock *f* shut, and the cocks *n o* open, the least alteration that happens to the air in the inside of the receiver, with respect either to encrease, decrease, density, or rarefaction, will be shewn either by the mercury or water, in either one or both gages; if considerable, the mercury will shew it, and if too little to affect the mercury, the water gage will shew it: if by any process the temperature of the air within the receiver should be raised, it will cause the air to expand, and the mercury and water in the gages to move towards *u u*, but if it should be lowered, the mercury and water will move towards *i k*; so likewise if any alteration, with respect to temperature, happens in the surrounding air, the gages will shew it accordingly; and as this is continually changing, it is necessary that the apparatus, after is prepared for the experiment, and ready for exploding of the wire, should be set in some medium which is not so changeable. For this purpose water seems to be the most convenient; but as it is a conductor of electricity, the apparatus must not remain in when the discharge is given through it; so that before, and after the discharge, it must only

be set into the water, care being taken to let it remain a sufficient length of time that the air in the inside may become of the same temperature, which will be shewn by the gages being stationary. If this circumstance be not observed with great attention, errors in the conclusion will be the consequence.

EXPERIMENT I.

A sufficient quantity of leaden wire, of $\frac{1}{50}$ th part of an inch diameter, was bound to a cord at different places about four inches distant, as has been already mentioned, and wound upon the axis of the wheel in *p q*, the end of it, by means of the long brass needle, was drawn out at the top of the receiver through the tube *m*, which was screwed in its place, and the cord drawn tight, as represented at *b*. The cock *f* was then closed, and the whole instrument set in the water so deep, that the brass plate was about one inch under the surface of the water. The cocks *n o* were opened to admit the external air into the receiver, as the air within became condensed by the coldness of the water. When it had remained nine minutes under the water, the gages were screwed on to the cocks perfectly air tight, and both appearing stationary, the cocks were shut. The instrument was then taken out, wiped dry, and set as near the electrometer, Fig. 2, as possible, not to influence the motion of the ball *A*, namely, at the distance of about one foot. One end of a wire, *a b*, was hooked to the cock *n*, and the other to the hook of the electrometer.* One end of another brass wire, *s t*, was hooked to the cock *f*, and the other end made to touch the outside of the battery, Fig. 1, which is composed of fifteen jars, containing about seventeen square feet † coating, and connected with the electrometer by means of a wire *L*. The battery was charged till the ball *A* of the electrometer, which was loaded with twenty-six grains, descended and directed the discharge through the wire *b*, which was converted into a blueish white impalpable powder, at first appearing like a white cloud, and filling the whole receiver (the cord was left entire); the instrument was then released from the electrometer, and set in the water as before. After it had remained there five minutes, the cocks under the gages were opened. No change could be perceived in the mercury gage, but a motion in the water gage was just perceptible. Hence I concluded that the powder thus produced had absorbed no air.

EXPERIMENT II.

The apparatus was taken out of the water, and another length of wire drawn forward, and placed at the electrometer and battery as before; the battery was charged till the electrometer caused the discharge, which again produced the powder. The apparatus was not put into the water as before, but was left to remain till another length of wire was drawn forward, and the discharge directed through it. And this process was repeated in the

* For a description of this electrometer, see this Journal, II. 528.

† For an account of this battery, see Journal, II. 526.

whole by five discharges through five lengths of wire, each length was seven inches. In this manner, therefore, thirty-five inches of wire was converted into a blueish white impalpable powder. The apparatus was then placed in the water as before, and after it had remained the same length of time, the mercury gage was opened. No motion in the mercury was perceived, and when the water gage was also opened, no alteration was there seen. Hence I concluded that no absorption of air had taken place, though five times the original length of wire had been reduced to powder. The air in the apparatus was then eudiometrically examined, and it appeared to be of the same purity as the surrounding atmosphere.

EXPERIMENT III.

The apparatus was taken out of the water, and the powder which was produced in the receiver carefully taken out; the apparatus was then furnished with the same length of wire as before, all the screws and cocks were well examined, and the apparatus set in the water as before. After it had stood its time, the gages were screwed tight, it was taken out of the water and set to the battery when the last experiment was repeated, and it was afterwards set in the water, and the gages opened. No absorption having taken place, it was left in the water till the same hour on the following day, when the mercury gage was opened, and indicated that an absorption of about one cubic inch had taken place. The water was of the same temperature as at first. This experiment consequently shews that the metal was first divided, and afterwards oxygenated by a slow process.

EXPERIMENT IV.

The last experiment was repeated with double the length of wire, and the result was the same.

EXPERIMENT V.

A smaller receiver was now taken, being only four inches diameter, which was half the diameter of the former. The same kind of powder was produced, and a diminution of air happened when thirty-five inches had been reduced to powder, which caused the water gage to rise one inch, and the mercury $\frac{1}{20}$ inch.

EXPERIMENT VI.

Wires of different diameters were now prepared in the last receiver, of $\frac{1}{100}$ and $\frac{1}{140}$ inch diameter, and of each thirty-five inches; and the result was the same with respect to air, but the powder was whiter.

By the result of the three last experiments, it appeared that the absorption of air by the exploded metal apparently depended upon the diameter of the receivers wherein the process was carried on, for which reason I determined to repeat the experiments in receivers of much less diameter.

EXPERIMENT VII.

Instead of the last-mentioned receiver, Fig. 4, was taken a glass tube, of about one inch diameter, and nine inches long,* and furnished with lead wire of $\frac{1}{8}$ inch diameter, being the same as that used in the three first experiments. After it had been placed in water, with the cock only above the surface and open; and it had remained three minutes, the cock was shut, the instrument taken out and wiped dry, one end of it was annexed to the chain *b*, of the electrometer, and the other end to the chain which was in communication with the outside of the battery, the same strength of charge was directed through the wire, by the electrometer, as in the last experiments, and the same kind of powder with respect to colour appeared; the instrument was again laid in the water, with the cock above the surface, and the water gage was screwed on to the cock; when three minutes were expired, the cock was opened, and there appeared so extraordinary diminution of air, that the water in the gage run over the upper bend into the instrument: this diminution appeared sufficient to make a mercury gage sensible.

The instrument being cleaned, and provided with thirty-five inches of wire of the same diameter as above, five separate discharges were directed through the wire; seven inches were exploded at each discharge, and the same process with respect to laying of it in water before and after each discharge was observed; and the

	Inches.
First discharge caused so extraordinary diminution, that the mercury rose in the gage	1
Second discharge	$1\frac{3}{10}$
Third	$0\frac{5}{10}$
Fourth	$0\frac{5}{10}$
Fifth	$0\frac{3}{10}$
In the whole	$3\frac{6}{10}$

which is nearly equal to two cubic inches. The contents of the whole instrument was nine cubic inches.

The air thus diminished in the instrument was left to stand till the same hour next day; but on opening the cock no further diminution had taken place.

The purity of this residue of air was tried by the test of flame, and it extinguished a candle eight times. Azotic gas, in the same glass, extinguished the same candle ten times, so that all the oxygen gas contained in that quantity of atmospherical air was nearly absorbed; the powder produced was nearly same as that in the large receiver.

* It was mounted in the same manner as Fig. 3, excepting that on account of its diameter only one air cock and gage was used.

This last experiment was repeated with wires of a less diameter, with nearly the same result; the five discharges produced a diminution, which caused the mercury to rise between three and four inches.

The result of the last experiments induced me to try receivers of a still less diameter; so one of $\frac{1}{16}$ inch diameter, and eight inches long, was taken; but on account of its smallness of diameter, no more air could be exploded than the length of the tube, without admission of fresh air.

EXPERIMENT X.

Accordingly, one discharge of the same degree of force was directed by the electrometer through one length of wire $\frac{1}{32}$ inch diameter.

	Inches.
The mercury gage rose - - - - -	$0\frac{3}{8}$
Second discharge through wire of $\frac{1}{64}$ - - - - -	$3\frac{3}{8}$
Third - - - - - $\frac{1}{83}$ - - - - -	$3\frac{3}{8}$
Fourth - - - - - $\frac{1}{158}$ - - - - -	$3\frac{3}{8}$

The wire through which the first discharge was directed, was only converted into a numberless small particles, retaining their metallic lustre. The second, which shewed the greatest degree of absorption, was but just converted into powder, mixed with particles of lead. The residue of this air was tried by flame, in a glass five inches high, and one and a quarter diameter, but at the mouth only three quarters, and the candle was extinguished in it nine times.

These experiments, I think, will be found sufficient to shew that the smaller the diameter of the receiver in which lead is calcined by an electric discharge, the greater proportion of air will be absorbed, and the ninth and thirteenth experiment proves that, that which is absorbed is the oxygen gas, which is by the process separated from the azote of the atmosphere.

It is well known that metals require certain degrees of temperature to give them the property of absorbing the greatest quantity of oxygen gas; this is the point which I had in view when I used wires of different diameters, because I know no way how to increase the temperature communicated by an electric discharge to metallic wires, but by diminishing these diameters or lengths in a certain proportion, or increasing the force of the discharge, which is the same thing, and the last experiment shews that it makes very little difference whether wire of $\frac{1}{64}$, or $\frac{1}{128}$, which is only half that diameter, be used, so that it does not seem here to follow the same law as common heat.

EXPERIMENT XI.

To conclude with this metal; the first-mentioned large receiver was taken, and 130 inches of wire, of $\frac{1}{83}$ of an inch in diameter, was prepared in the same, being nearly four times

times the quantity that was used before. This was exploded and converted into an impalpable powder by twenty separate discharges, and when the gages were opened, there appeared to have been no air absorbed.

EXPERIMENT XII.

The powder or calx last produced, was preserved and put into a very small glass retort, with an intent to try what would happen, or what alterations would take place by the application of heat. No air was given out by such treatment, excepting a little common air, which I suppose was contained in the retort. The powder, which was first of a blueish white, changed into a dark leaden colour, and afterwards into a yellowish green; and when the glass was red hot, nearly melting the powder, was only partially melted into lead.

Lastly, twenty-four grains of this powder, which had absorbed no oxygen, was taken and put into a clean crucible, with a cover to it, set in a furnace, and kept in a white heat about one hour. When taken out, there was found in the bottom of the crucible a perfect clear glass of a redish brown colour. Thus we find a powder obtained which possesses the most distinguishing properties of an oxide of lead, though it appeared to have absorbed no oxygen in its preparation.

Lead burns with a yellowish flame by the electric discharge.

These experiments appear to be sufficient to prove, that the absorption of the oxygen gas from the atmospheric air, by the powder thus produced, wholly depends upon the diameter of the receiver wherein the process was carried on. For we find by the tenth experiment, that when eight inches of wire were calcined in a receiver only $\frac{4}{10}$ of an inch diameter, it then absorbed half a cubic inch of air. In this proportion the wire last calcined should have absorbed sixteen times that quantity, and the result was that it had absorbed none; the reason of which I must own I do not understand.

These experiments were made in the winter season, when the temperature of the atmosphere was seldom lower than 28° , and not higher than 45° , and the result was as mentioned; but not being able to reconcile myself to such paradoxical results, I showed the experiments to several philosophers, who, however, were equally as much at a loss to account for the result as myself.

On the 10th of May, 1798, I was favoured with the company of some gentlemen of the Royal Society to see the experiments. I used only two receivers, one of an inch diameter, and the other of six inches diameter, as follows:

EXPERIMENT XIII.

A piece of leaden wire, of nine inches long, and $\frac{1}{8}$ of an inch in diameter, was exploded by an electric discharge in the receiver of one inch diameter. After it had laid in water three minutes, the mercury gage rose $\frac{13}{20}$ of an inch, which was a degree of diminution of about half a cubic inch.

EXPERIMENT

EXPERIMENT XIV.

Eight lengths of wire, of the same diameter, was exploded in the receiver of six inches diameter; and after it had been in water five minutes, the mercury gage rose $\frac{2}{10}$ of an inch, which was a degree of diminution of about $3\frac{1}{8}$ cubic inches, and consequently not agreeing with my former experiments.

The result of these two experiments occasioned several repetitions, and all nearly with the same result; but on repeating them in the winter, when the temperature was low, I found the result as I first had (namely) a diminution of air in the small receiver, and none in the large one. It then occurred to me that the temperature must have occasioned this variation, and that putting the receiver in water for five minutes was, perhaps, not sufficient to bring the air in the inside of the receiver to its former temperature. I therefore placed a thermometer in the inside of the large receiver, and by exploding one length of wire, the temperature was raised about two degrees; and when the receiver was set in water, the air did not return to its former temperature in less than twenty minutes; consequently this pointed out to me one error, which I was not aware of; but it was not sufficient to explain the whole, because this only would indicate a diminution but less than truth. By repeating the experiment at different times, I perceived that the first explosion in a low temperature caused the mercury to rise near two degrees, and that every subsequent explosion affected it less. After seven or eight explosions had been given, hardly any rise in the mercury could be perceived. And also when the temperature was at about 60° , the first explosion did not then cause the mercury to rise to above 1° , and it would cease to rise after the third or fourth explosion. When the temperature was at about 70° ; hardly any rise in the mercury could be perceived, even at the first explosion; this unfolded the whole, because it appeared that when the temperature was low, the explosions raised it so much as to cause an expansion in the inclosed air, which kept up the pressure upon the mercury in the gages, and of consequence hindered them from indicating any loss; and when the temperature was high, the explosions did not cause such expansion, consequently permitted the gages to indicate the true loss, or quantity of air which had been absorbed by the divided metal.

I thought it proper to mention the above in full, not only for the advantage of such as might undertake such experiments, but likewise to prevent wrong conclusions being drawn from the result of the first set of experiments with large receivers, as they have been shewn to many, and some authors have brought them forward to support their own erroneous notions.

We may also conclude from the facts that the difference of diameters of the receivers wherein leaden wire is exploded, neither favours, nor hinders, the diminution of the atmospheric air therein contained. If two leaden wires of equal lengths and diameters be exploded by equal electric discharges, in unequal receivers, the diminution of the air

which

which they contain will be equal in both; and if the receivers contain atmospherical air, that which is seemingly lost by the process is oxygen gas, absorbed by the exploded metal, which becomes converted into a true metallic oxide.

If it be required to collect the produce for further investigation, a receiver of the largest diameter then becomes necessary, because the oxide is more perfect, and a greater quantity of powder can be produced, and the experiment oftener repeated, before the receiver becomes opaque, which is the case when receivers of small diameters are used.

The foregoing experiments upon lead, prove that a receiver of one inch diameter is sufficiently wide, and therefore preferable to wider, because when the temperature of the air in the inside is raised by the explosions, it is much sooner brought to its former temperature, and is not so liable to equivocal results. I have not, therefore, thought it proper to relate the experiments which were made in wider receivers upon the following metals: such receivers being necessary only when a large quantity of oxide is required.

EXPERIMENT XV.

Tin.

Sixteen inches of tin wire, of $\frac{1}{120}$ part of an inch in diameter, was put into Fig. 4, and after it had been laid in water so long, that the air in the inside was become of the same temperature as the water itself; it was afterwards exploded by two discharges, and by that means converted into an impalpable powder, nearly the same as the lead wire, but of a purer white. The apparatus was then again laid in water * till the air had gained its former temperature; when the cock was opened, the mercury rose three inches, and the air, by the test of flame, seemed to be as highly azotic as that in which lead had been exploded in the ninth experiment.

The last experiment, repeated with wire of $\frac{1}{100}$ of an inch diameter, and the result was the same.

Tin burns with a vivid yellow flame when exploded.

EXPERIMENT XVI.

Iron.

Thirty-two inches of iron wire, of $\frac{1}{230}$ inch diameter, was put into Fig. 4, and exploded by five discharges: the mercury rose three inches.

The wire was converted into an impalpable powder, of a reddish brown colour; the residue of air seemed to be the same as that left in the ninth and fifteenth experiments, wherein lead and tin had been exploded. It extinguished a candle nine times.

* The apparatus was always laid in water a sufficient time to cause the air in the inside to become of the same temperature as the water, before and after the metal was exploded; but to avoid prolixity I have not mentioned it in the relation of the following experiments.

EXPERIMENT XVII.

Wires of different diameters were taken, with a view to try what degree of fusion was required to cause the greatest absorption of air, or, in other words, to make the most complete oxide.

The same degree of strength of charge was directed through wire of $\frac{1}{100}$ inch diameter; no air absorbed; the wire was only red hot.

Wire of $\frac{1}{100}$	was exploded and converted into balls;	the mercury rose	$0\frac{5}{10}$
— $\frac{1}{30}$	converted into very small powdered balls	- - -	$0\frac{6}{10}$
— $\frac{1}{80}$	into flocculi and balls	- - -	I
— $\frac{1}{40}$	into an impalpable powder of reddish brown colour	- - -	$1\frac{1}{10}$

EXPERIMENT XVIII.

Copper.

Thirty-two inches of copper wire, of one 260 part of an inch diameter, was prepared in Fig. 4, and exploded by five discharges; the mercury in the gage rose $1\frac{1}{10}$ inches.

It was converted into an impalpable powder, of a purple brown colour; the residue of air extinguished the flame of a small wax taper twice.

This metal burns with a green flame when exploded.

All the foregoing experiments were made by myself, at my house in Poland Street; but the following were made at the house of Mr. Campbell, in South Audley Street, who honoured me with his assistance. The apparatus we used were of my own construction, being a double plate machine; each plate twenty-six inches diameter, and a double battery of thirty jars of my common size, containing about thirty-four square feet coated glass. The electrometer was the same as I have described in Nicholson's Journal, Vol. II. page 528, excepting that instead of using pins of certain weights to put into B, the arm C B was divided into grains, with a slider upon it, which was set to the weight required, as I commonly make them at present; being more convenient.

EXPERIMENT XIX.

Pure Silver.

Forty-five inches of silver wire, of $\frac{1}{30}$ part of an inch diameter, was exploded by five discharges; the slider upon the electrometer was set at forty grains; it was converted into an impalpable black powder: the mercury rose in the gage about half an inch, and the flame of the taper was extinguished nine times.

Silver burns with a greenish flame, nearly resembling that of copper.

EXPERIMENT XX.

Grain Gold.

Fifty-four inches of wire drawn from grain gold, of $\frac{1}{125}$ part of an inch diameter, was exploded by six discharges; the slider upon the electrometer was set at forty-two grains; it was converted into an impalpable powder, of a dark brown purple colour: the mercury rose in the gage about a quarter of an inch, and the flame of a taper was extinguished four times.

EXPERIMENT XXI.

Nine inches of the same sort of wire, but of a less diameter, being $\frac{1}{30}$ part of an inch, was exploded in the same receiver by one discharge: the mercury rose about $\frac{2}{10}$ of an inch, and the flame of the taper was extinguished four times.

EXPERIMENT XXII.

Platina.

Twenty-seven inches of platina wire, of $\frac{1}{30}$ part of an inch diameter, was exploded by three discharges. When the gage cock was opened, the mercury rose a quarter of an inch, but on applying the flame to the residue, it was not extinguished.

As platina is easily fused by electricity, the slider upon the electrometer was only set at twenty-two grains for the last experiment, and the residue of air did not extinguish the flame. It was supposed that too low a charge had been used. The slider was therefore set at thirty-two grains, and the experiment was repeated with the same length and diameter of wire, and exploded by the same number of discharges; when the cock was opened, the mercury rose about one inch, and the flame of the taper was extinguished five times: the metal was converted into a black impalpable powder, resembling that of silver.

EXPERIMENT XXIII.

Zinc.

Nine inches of zinc wire, of $\frac{1}{30}$ part of an inch in diameter, was exploded by one discharge; when the gage cock was opened, the mercury rose half an inch, and the residue of air extinguished the flame of a taper twice: the metal was converted into an impalpable powder of a white colour, resembling that of tin.

This metal being so difficult to draw into wire, I was obliged to conclude with the above-mentioned length.

From the result of the foregoing experiments, it may be safely concluded, that all the ductile metals can, by electric discharges, be sublimed and converted into proper oxides, by absorbing the oxygen of the atmosphere; and though some of the metals resist the action of common fire, and require different solvents to convert them into oxides, yet they all yield to the action of the electric fluid.

It is remarkable that platina, though it resists the action of common fire, is more easily fused by electric discharges than copper, silver, or gold, and seems to be as greedy of oxygen as any of the other metals. But these experiments have not been sufficiently extended to settle the last mentioned property.

It is well known that all metals which are sublimable by common fire, absorb oxygen in different degrees, and likewise in different proportions, according to the different degrees of heat employed; this seems likewise to take place when they are sublimed by electric discharges, but the proper degree of discharge for each metal remains for investigation; and as different metals differ as conductors of the electric fluid, (which has never yet been well defined) this furnishes matter sufficient for a future pursuit.

The latest experiments upon the conducting power of metals were made by Dr. Van Marum and myself, at Haerlem, but as I had not at that time invented the electrometer which I have at present, and which the nature of the experiment required, I have some reason to think that they are not perfectly accurate. I have concluded these experiments, without proceeding to the nonconductive metals, because I know no way of arranging the others, so that they can be acted upon with sufficient accuracy.

The experiments which have been made upon the different metals mentioned, have been very numerous, excepting zinc, but for the sake of brevity, and to avoid tautology, I have only mentioned those which I thought to be absolutely necessary.

V.

*Description of an Improvement in the Steam Engine, by means of which the Boiler is constantly supplied with Water already heated nearly to the point of Ebullition. Communicated by Mr. PETER KEIR, Engineer.**

THE steam engine, of which we have so many accounts in introductory and other works, and which for its scientific contrivance and extensive utility cannot be too highly regarded, may be traced in its history through a few great and striking degrees of improvement, the intention of every one of which consists in producing the greatest quantity of moving force with the least possible consumption of fuel. It is probable that the original engine of the Marquis of Worcester may have been a mere vessel into which water flowed by its weight, and was thence driven upwards by steam: Capt. Savary seems to have been the first who used the vacuum formed by steam; into which the water was urged by the atmosphere: Papin in a crude way, and Newcomen and Cawley in a more perfect method, used a solid piston: and lastly, James Watt greatly improved the engine of the latter op-

* In conversation. I am very sorry that the lateness of the date of this sheet at press, does not allow me time to send and inquire the name of the inventor, which I omitted to write down. The improvement is under patent, which, with the name, I shall mention in my next.

rators by condensing his steam in a cold vessel at a distance from the working cylinder. The other improvements in the mechanism in the furnace, in the double stroke, &c. &c. though highly valuable and honourable to the skill and contrivance of the inventors, appear to be of subordinate importance, and in many particulars are little more than those variations of fashion which in mechanics, as well as other less important departments of human industry, are more or less prevalent.

It was among the early improvements of the steam engine, to feed the boiler out of that hot water which was afforded by the condensation of the steam along with the stream of injection. When the boiler receives a quantity of cold water, it becomes necessary either to increase the fire, or to admit the engine to work with less effect from the diminished quantity or elasticity of the steam produced; and when the water thus supplied is afforded in the manner just mentioned, it will be hotter the less the injection, and the contrary. But there is a mean which must be attended to in the adjustment of this heat. A sparing injection will not afford a good vacuum, and therefore the engine will work with diminished effect; and a copious injection will afford water at a low temperature to the boiler. When the adjustment is at the best, the water of the supply is not hotter than the hand can bear, that is to say, about 120° . It becomes therefore an object of value, to ascertain the means by which the condensation of the steam may be rendered as perfect as the introduction of cold water can make it, and at the same time, that a greater part of the heat it imparts may be employed in raising the temperature of the water intended for the boiler.

Plate VIII. represents a section of the parts of a double engine; that is to say, an engine in which the piston is alternately impelled by the elasticity of steam acting above and below. A A is the great cylinder in which the piston moves; O, P, are the steam boxes, or metallic receptacles, into which the steam is first introduced from the boiler by pipes at O and P, not represented in the figure. They contain each two valves, B, C, and *b*, *c*, which are opened or shut by the usual apparatus attached to the working beam; *d*, *d* is the passage to D D the condenser, terminating in H the air pump. E F G are the additional parts of which the new improvement consists.

Let us then suppose the engine in actual work, the valve *b* in the lower steam box being open, and *c* shut, while B in the upper steam box is shut, and C open. From this disposition steam will rush in from the boiler through *l*, beneath the piston, while the other steam which had been admitted before above the piston, will have a communication through L, C, K and *d*, to D the condenser, where a constant injection J being kept up, it becomes condensed and leaves a vacuum. Consequently the piston must rise by the whole force of the steam beneath. When it has arrived near the top of the cylinder, the valves in the steam boxes *b* and C are shut, and the others B and *c* are opened; so that the steam from the boiler rushes through L into the space above the piston, while the other steam beneath passes through *l*, *c* and *k* to the condenser, where it assumes the dense form and leaves a vacuum, into which the piston is urged in its descent. When the descent is completed,

the

the valves resume their first state, and the piston must again rise, &c. &c. The pump H is kept constantly at work by the motion of the engine, and carries off the water of the injection heated by the steam, and also the permanently elastic fluid, which may be extricated from the water: and of this hot water it is that the boiler in steam engines is usually replenished. Instead however of this direct supply in the present engine, the hot water is suffered to flow into the smaller reservoir G, G, G, which it keeps constantly full and overflowing, and from which a portion is raised by the small pump E through the pipe F, M, F, F, transmitted through the passage to the condenser, and consequently having its exterior surface in contact with the hot steam as it rushes towards the condenser, and by that means having its contents, and consequently those of the vessel G G G, greatly heated by the circulation effected by the pump. The boiler is supplied by the pipe N, placed so far below the return M, that the pressure of the column I M may exceed the re-action of the steam in the boiler, which might otherwise prevent its introduction.

I am not prepared to say to what extent this improvement may add to the power of the engine.

VI.

*Inquiries relative to the Laws of Affinity. By CIT. BERTHOLLET.**

(Continued from page 108.)

On the Influence of the Proportions in Complex Affinities.

IN my former inquiries I have ascertained the principal results afforded by complex affinity, when the force of cohesion, or that of elasticity are sufficiently great to produce that change of base which had been attributed to the superiority of the divellent affinities over the quiescent affinities. But I have neglected to enter into the necessary details for determining the changes which may arise from different proportions of the substances which are brought into action, when the force of cohesion is not sufficiently great to cause the effects of this difference to disappear. I have promised (in Art. XII. No. 6,) to return to this subject, which I shall now proceed to do.

According to the theory which I have laid down (Art. V. No. 5), all substances exert a mutual action during the time they are in the liquid state; so that in a solution, for example, of sulphate of pot-ash and muriate of soda, these two salts are not distinct, while there is no cause to determine the separation from their combination; but that there exists in this liquid, sulphuric acid, muriatic acid, soda, and pot-ash. I shall, however, continue to make use of the ordinary language, which after this notice, ought not to produce any ambiguity.

* Annales de Chimie, XXXVIII. 3.

2. I begin with mixtures in which a considerable force of crystallization must determine the combinations which are formed.

Experiment A. Equal parts of nitrate of lime and sulphate of pot-ash were mixed: after the separation of the sulphate of lime which was first formed, and of which I shall say more in the following experiments, the liquid was evaporated, and nothing was obtained by the successive operations but nitrate of potash and sulphate of lime. After the last evaporation, however, some crystals of sulphate of pot-ash were procured; there remained but a very small quantity of uncrystallizable liquid, which precipitated with the carbonate of soda and with the nitrate of barites; so that it consisted of a little sulphuric acid and lime, and very probably a larger portion of nitrate of pot-ash.

The quantity of sulphate of lime which was deposited in the course of the evaporation, was much more considerable than would have been obtained from simple solution of this salt in water; so that its solubility was augmented by the action of the other substances.

Experiment B. Two parts of sulphate of pot-ash, and one of nitrate of lime, gave by a first evaporation sulphate of potash and sulphate of lime; and by the succeeding evaporations, nitrate of pot-ash with the two sulphates, the proportions of which continued to decrease to the last crystallization. There remained only some drops of the liquid uncrystallized; they did not precipitate with the carbonate of soda, but did with the nitrate of barites: hence they were probably formed of sulphate of pot-ash, and a small proportion of nitrate of pot-ash.

Experiment C. Two parts of nitrate of lime, and one part of sulphate of pot-ash, afforded during the first evaporation a small quantity of sulphate of lime, and by cooling, nitrate of pot-ash; the other evaporations only produced nitrate of pot-ash. In the latter, however, some crystals of sulphate of lime were perceived at the surface of the liquid. Notwithstanding the residue, which was abundant, was several times submitted to evaporation and cooling, it no longer afforded crystals of any salt. This uncrystallizable residue treated with alcohol, formed an abundant deposition, which having been dissolved in water, scarcely afforded any precipitate with the nitrate of barites; so that it contained little or no sulphuric acid, and consisted of pure nitrate of pot-ash: the portion dissolved by alcohol was nitrate of lime, with a small portion of nitrate of pot-ash. The uncrystallizable residue was therefore composed of nitrate of pot-ash and nitrate of lime.

In this experiment we see, that the sulphate of lime was rendered much less soluble than in the preceding experiments; but that a considerable quantity of nitrate of pot-ash lost the property of crystallizing by the action exerted on it by the nitrate of lime.

3. In these three experiments sulphate of lime must have been formed, because the lime and sulphuric acid coming in contact, would separate on account of the insolubility which belongs to their combination.

The sulphate of lime in the experiment A and in the experiment B, was rendered much more soluble than it naturally is, by the action of the substances which were in solution; but in the experiment C, its solubility was not sensibly increased, probably because the
nitrate

nitrate of lime and the nitrate of pot-ash which formed the uncrystallizable liquid, mutually experienced a degree of saturation, which considerably weakened their action upon the sulphate of lime.

4. From these considerations, I shall proceed to deduce first, the theory of the uncrystallizable residues which are found in the solutions of salts which are evaporated: it will be confirmed by the succeeding observations.

Saline substances exercise upon each other an action which increases their solubility, an effect that has been particularly established by the experiments published by my learned colleague Vauquelin (*Annales de Chimie*, tom. xiii.) This mutual action varies in the different salts. It has however been thought, that salts with an earthy base do not increase the solubility of the nitrate of pot-ash, though these in reality increase it the most.

There is doubtless in this respect, a difference in the effect produced by the salts, which depends on their nature; but this difference is in general very small, compared with that which proceeds from the force of crystallization.

Experiment D. A mixture of equal parts of nitrate of pot-ash and sulphate of pot-ash, afforded by evaporation, successively and in proportion to their solubility, sulphate of pot-ash and nitrate of pot-ash, without leaving any uncrystallizable liquid; but the same experiment being made with a mixture of nitrate of soda and sulphate of soda, both of which have only a slight tendency to crystallize, and are nearly of equal solubility, only a small quantity of sulphate of soda is separated by crystallization, all the rest remaining liquid, without any crystallization. A mixture of muriate of soda and sulphate of alumine having been submitted to the same proof, it was clearly perceived that the two salts had become more soluble; but they were intirely separated by the alternate evaporation and cooling.

It is therefore seen that substances which possess a considerable force of crystallization, though rendered more soluble, separate on account of their insolubility, and leave very little or no uncrystallizable residuum.

But when salts are found which have only a feeble disposition to crystallize, their mutual action counterbalances their force of crystallization, so that there then remains much liquid which cannot crystallize; and this happens particularly when the residuum contains a substance in itself uncrystallizable; as in experiment C, where by the proportions employed, there was found a superabundance of nitrate of lime, which by its action upon the nitrate of pot-ash reduced a considerable quantity of that salt into an uncrystallizable liquid.

Whence therefore does it happen, that the most learned chemists, Lavoisier, Fourcroy, Vauquelin, Guyton, and the Commissaries of the Academy of Sciences, among whom myself was one, could have been led by the experiments made upon the proof of salt-petre, to believe, that the nitrate of lime exerts no action upon the nitrate of pot-ash, nor increased its solubility? (*Ann. de Chimie*, tom. xi. xiii. xv. xxiii.) It was because, in the experiments that were made, a water of nitrate of pot-ash was put to digest upon dried nitrate of lime. The latter necessarily caused a division of the water upon which it has a
strong

strong action. It would therefore have precipitated a considerable quantity of nitrate of pot-ash, if this effect had not been compensated, or nearly so by the solubility which it imparted to that salt; but if the solution had been evaporated, there would have been extracted by crystallization much less nitrate of pot-ash than was contained in the liquid, and an uncrystallizable residuum would have been left similar to that of experiment C. The proof therefore which might be considered as conclusive for the object proposed, has induced an error respecting the chemical phenomenon.

Hence it follows, that when the mother waters of the salt-petre works are decomposed by pot-ash, the nitrate of pot-ash obtained consists not only of that which has just been formed, but also that which was rendered uncrystallizable by the salts with an earthy basis.

5. I made mixtures of crystallized sulphate of soda and nitrate of lime. Here the sulphate of soda, and the nitrate of soda, which might proceed from the operation, differed little in point of solubility, and had a crystallizing power less considerable than the sulphate and nitrate of pot-ash.

Experiment E. Equal parts of crystallized sulphate of soda and nitrate of lime, reduced to the state of dryness, gave by evaporation only a small quantity of nitrate of soda. The uncrystallizable residue, which was abundant, did not precipitate with the muriate of barites, but with the oxalic acid.

Experiment F. Two parts of sulphate of soda, and one part of nitrate of lime, afforded after evaporation, a greater quantity of nitrate of soda than the preceding experiment; the uncrystallizable residue did not precipitate with the oxalic acid, but with the muriate of barites. In order to compare the two last experiments with the experiments A, B and C, it must be observed, that the crystallized sulphate of soda contains more than half its weight of water of crystallization.

Even in experiment F, no sulphate of soda is formed, as sulphate of pot-ash was formed in experiments A and B, because the sulphate of pot-ash has a force of crystallization much greater than the sulphate of soda.

The residue of experiment E did not contain such a quantity of sulphuric acid as could be rendered perceptible by the muriate of barites; but it was formed of nitrate of soda and nitrate of lime, which probably, by their mutual action, so much exhausted their dissolvent power, as not to prevent the separation of the sulphate of lime No. 3.

In experiment F the residue was not precipitated by the oxalic acid, but by the muriate of barites; so that this residuum was composed of sulphate of soda, and a greater proportion of nitrate of soda, which mutually prevented their crystallization, as in experiment D.

6. *Experiment G.* Equal parts of nitrate of pot-ash and sulphate of soda, gave by successive crystallizations, 1. sulphate of pot-ash, and some small crystals of nitrate of pot-ash; 2. a little sulphate of pot-ash, and a greater proportion of nitrate of pot-ash; 3. small crystals of nitrate of pot-ash, and much nitrate of soda; there was an uncrystallizable residue, notwithstanding

notwithstanding the care which had been taken to obtain the utmost crystallization; this residue was formed of nitrates and sulphates; for it precipitated abundantly with the nitrate of barites, and after drying it flowed on ignited charcoal. This experiment ought to be compared with experiment F.

Experiment H. One part of nitrate of pot-ash, and two of sulphate of soda, gave, 1. sulphate of pot-ash; 2. sulphate of pot-ash, and some needles of nitrate of pot-ash; 3. sulphate of pot-ash in small prisms, suspended from a pellicle formed by nitrate of soda, fine crystals of nitrate of pot-ash and nitrate of soda; the residue contained nitrates and sulphates.

In these two experiments, the least soluble salt of those which were formed, viz. the sulphate of pot-ash, was the first which crystallized. When the proportions had been thus changed, the action of the nitric acid on the pot-ash resumed its superiority, and nitrate of pot-ash was formed, though the liquid still contained sulphuric acid.

In the second, the more abundant sulphuric acid gave rise to a greater quantity of sulphate of pot-ash; but after the first crystallization, more nitrate of pot-ash was formed, though there would have been a sufficient quantity of sulphuric acid to cause a complete change of base, if this exchange could have been made as has been imagined. The residue was formed even in experiment G, of sulphate of soda and nitrate of soda; and probably of a small quantity of salt with base of pot-ash.

7. *Experiment I.* Equal weights of nitrate of pot-ash and muriate of lime were mixed together: by evaporation there were obtained, 1. nitrate of pot-ash; 2. muriate of pot-ash, in which was found a little nitrate of pot-ash: the residue gave with the sulphuric acid an abundant precipitate of sulphate of lime, and vapours of muriatic and nitric acid were disengaged.

Experiment K. The experiment having been made with two parts of muriate of lime, and one of nitrate of pot-ash, an abundant crystallization was effected of muriate of pot-ash with no appearance of nitrate; the residue treated as in the preceding experiment, gave analogous results.

Experiment L. A mixture was made the reverse of those of the preceding experiments; it was formed of equal parts of muriate of pot-ash and nitrate of lime. It afforded, 1. nitrate of pot-ash, mixed with a little muriate of pot-ash; 2. muriate of pot-ash, which was mixed with a small quantity of nitrate of pot-ash. The uncrystallizable residuum was dissolved in alcohol; there was a separation of nitrate of pot-ash, which was fused upon ignited charcoal, but contained a little muriate of pot-ash, as was seen by the test of the solution of silver. The sulphuric acid shewed, that the part dissolved by the alcohol contained muriatic acid, nitric acid, and lime.

In the experiments above described, and in which substances were employed whose combinations could not have a considerable force of crystallization, and differed but little among each other in this respect, it is evident, that the formation of the salts which were afterwards obtained by crystallization, depended on the proportions of the substances which mutually acted among each other. In experiment I, which might afford nitrate and muriate of pot-ash; as these two salts differed little in their solubility, which however is rather

the least in the latter, the nitrate of pot-ash was obtained by the first crystallization; but as in experiment K the muriatic acid existed in a greater proportion, muriate of pot-ash only was then obtained: one part of pot-ash with muriatic acid, nitric acid and lime formed the residuum. The proportions employed in experiment L, differed little from those of I, and the results were nearly the same.

Opposite combinations therefore are obtained according to the proportions employed, or according to the period of crystallization; that is to say, according to the proportions of the substances which remain in action, when a sufficient force of cohesion does not exist in the combinations that might be formed. But according to the opinion adopted by chemists, the entire change of base which it is affirmed must take place, may be judged of even by a first crystallization.

If, for example, nitrate of pot-ash be first obtained from the proportions which have been employed of muriate of pot-ash and nitrate of lime, a conclusion is made, that an exchange of base has been effected between the muriatic and nitric acids. If other proportions had been employed, which would have afforded muriate of pot-ash, as in experiment K, an opposite consequence would have been drawn. Chemists have even gone farther: from the conclusion respecting the change of base, for example, between the muriatic and nitric acids, a farther inference has been made, that the combinations opposite to those which afforded such a change, would not be at all affected by their mixture.

8. I have examined what change might be produced in the results by a substance which has the property of forming triple salts, such as magnesia.

Experiment M. Equal parts of sulphate of pot-ash and muriate of magnesia, afforded, 1. sulphate of pot-ash; 2. sulphate of pot-ash, a little muriate of pot-ash, and a triple salt, composed of sulphuric acid, pot-ash, and magnesia; this salt forms fine rhomboids, which, by exposure to the air, do not lose their transparency; its solubility is nearly the same as that of the sulphate of pot-ash: 3. muriate of pot-ash and sulphate of magnesia. The residuum contained sulphuric acid, muriatic acid, pot-ash, and magnesia.

Experiment N. Two parts of muriate of magnesia, and one of sulphate of pot-ash, afforded, 1. sulphate of pot-ash; 2. muriate of pot-ash, and the triple salt of the preceding experiment; 3. muriate of pot-ash, and sulphate of magnesia: the residuum was analogous to that of the preceding experiment.

In experiment M, sulphate of pot-ash was obtained by two crystallizations; but in experiment N, where the muriatic acid was in a greater proportion, it was obtained on the first crystallization; the triple salt, which in solubility nearly approaches the sulphate of pot-ash, crystallized after the second evaporation. When the proportion of sulphuric acid was sufficiently diminished by these crystallizations, then the muriate of pot-ash was separated, nearly according to the order of solubility; and lastly the magnesia, which being still abundant, crystallizes with part of the sulphuric acid.

We see that a different opinion might have been formed in these two experiments, according to the period of the crystallization to which the attention might have been directed,

and how far distant from truth the established opinion is, that a complete change of bases takes place on the mixture of muriate of magnesia and the sulphate of pot-ash.

In experiment M, where the sulphuric acid was present in greater proportion, sulphate of pot-ash was obtained in both the first crystallizations; but in experiment N, where the muriatic acid had more influence by its relative quantity, sulphate of pot-ash was obtained only in the first crystallization; the magnesia, for the most part, remained in the un-crystallizable residue, because it has no force of crystallization with the muriatic acid, and only a weak power with the sulphuric acid; other salts which would in other circumstances have been crystallizable, are retained in the residue which opposes their crystallization.

The difference observed between the results of these two experiments, and those of the experiments A, B, and C, in which a deliquescent calcareous salt was put into action with the sulphate of pot-ash, corresponds exactly with the difference of solubility which exists between the sulphate of lime and the sulphate of magnesia.

9. After having treated of saline substances in the preceding experiments, as if they formed separate or distinct combinations in a liquid, I shall now consider some effects which are owing to the mutual action which is in reality exerted by them all when they are mixed in this state.

Experiment O. Aqueous solution of the acetite of lead was gradually poured into a solution of muriate of soda, till no more precipitate was produced. The liquid which remained above the precipitate strongly reddened blue paper, which effect was not produced by the mere solution of the acetite of lead, nor by the muriate of soda; but it assumed a deep colour with the hydro-sulphurets, and afforded an abundant precipitate with the muriatic and sulphuric acids. During its evaporation a deposition was formed of a scarcely soluble muriate of lead, and also a crust which had not a crystalline appearance, and which will be spoken of hereafter. Lastly, some fine crystals were obtained, which were an acetite of soda, and an oxide of lead. On dissolving the saline crust, a deposition took place of muriate of lead with excess of oxide, analogous to that described by Vauquelin (*Annales de Chimie*, t. xxxi.) It was necessary to repeat the solution and crystallization several times, before the deposition ceased. The saline crust was thus, or at this period, divided into two substances, muriate of soda, and muriate of lead.

If the muriatic acid remain engaged in the acid together with the acetous, as both acids are volatile, the excess which is but slightly engaged, and which may be expelled by the action of heat, must be composed of two acids. In fact, having distilled a mixture similar to the preceding, after having separated the deposition which was first formed, the liquid which passed into the receiver contained acetous and muriatic acid; this circumstance deserves particular attention, and serves to explain several observations.

10. Citizen Prieur has observed that when lead is employed for purifying the solution of silver mixed with muriatic acid, a part of the last-mentioned acid passed over by distillation. This happens because the muriate of lead is soluble, and even very much so by the action of the nitric acid. The liquid is therefore composed of oxide of lead, muriatic acid

and nitric acid. The oxide of lead divides its action upon the two acids, and both are subjected to the action of expansibility produced by heat.

If the sulphuric acid were to be retained, lead would be an efficacious medium; 1. because the sulphate of lead is much less soluble than the muriate; 2. because the sulphuric acid is much less volatile than the muriatic acid.

The muriate of silver being much less soluble than the muriate of lead, silver is much better than lead for retaining the muriatic acid which is found mixed with nitric acid. Velter and Bonjour have nevertheless observed that muriatic acid always came over in distillation, when the operation was made without the precautions pointed out by those learned chemists. To obtain a pure nitric acid immediately, it is necessary to make the operation upon an acid little concentrated, in order that it may not hold in solution any muriate of silver, and to separate the muriate of silver which precipitates before the liquid is subjected to the action of heat, or which is preferable, to precipitate by the solution of silver, the muriatic acid from the nitrate of pot-ash; after which, by decomposing this nitrate, the nitric acid is obtained perfectly free from muriatic acid.

When muriate of silver remains in the solution, a very pure nitric acid may nevertheless be obtained by distillation, by setting aside the first portion which distils over, till it is seen by the test that no more muriatic acid is afforded. For as the chemists I have just quoted observed, the muriatic acid in this process assumes the nature of oxygenated muriatic acid, and is disengaged in this state at the beginning of the operation.

Experiment P. Sulphate of pot-ash having been treated with the acetite of lead, in the same manner as the muriate of pot-ash, sulphate of lead was precipitated. The liquid retained only a small quantity of oxide of lead. By the progress of evaporation some crystals of sulphate of pot-ash were obtained, though before the evaporation the acetite of lead produced no more precipitate; and, lastly, the acetite of pot-ash, which retained a small quantity of oxide of lead. The decomposition of the sulphate of pot-ash was much more complete than that of the muriate of soda.

In the experiments I have just described, we see, then, that the decompositions, or exchange of bases, also follow the order of the solubilities of the combinations which may be formed, and that the only difference observed in most of the preceding experiments arises from the property possessed by the oxide of lead of forming triple combinations, which sometimes again separate in combinations of different degrees of solubility, as happens with the muriate of soda and of lead. *Experiment O.*

11. The following experiment will confirm the truth that the force of cohesion produces a different effect, according to the properties of the solvent.

Experiment Q. I mixed an aqueous solution of plumbate of soda with the water of sulphate of soda, a slight precipitate only was produced, though the sulphuric and muriatic acids, if added, would have produced an abundant precipitate: when the experiment was made with the muriate of soda, the precipitate was much more abundant than with the sulphate of soda.

On the first view, these effects may appear to be contrary to the principles I have established. For the sulphate of lead is much less soluble than the muriate of lead; so that it might be expected that a more abundant precipitate would be obtained in the experiment made with the sulphate of soda than in that with the muriate of soda. This happens otherwise because the sulphate of lead is much more soluble in soda, as I have ascertained, than the muriate of lead, with excess of oxide, such as is precipitated in the preceding experiments. And the precipitation is not a direct consequence of the force of cohesion, but arises from the excess of the force of cohesion beyond that of the solvent.

12. The observations which I have presented in this memoir may be reduced to the following results. In the complex affinities, or double affinities, the force of cohesion, when considerable, and differing much in its intensity among the combinations which may be formed, determines a change of bases, in such a manner that the most insoluble combination is formed and separated independently of the proportions, which have an influence only upon the state of those substances which remain in solution. The result of a mixture of different saline substances may therefore be foretold from the mere consideration of solubility.

In this case the adopted theory of the quiescent and divellent affinities does not mislead us, as to the principal result, that is to say, the formation of the insoluble salt. But as it is not deduced from facts of a superior order, it will demand as many experiments as particular facts. The doctrine is not established upon any foundation from which we can foretell the mutual actions of substances presented to each other; and again it has the disadvantage of affording no indication respecting the properties which the remaining fluid portion ought to exhibit when subjected to evaporation, or to the action of a new substance.

Though this certain relation in the results may be productive of doubt as to the theory in the foregoing case, it is not the same when there is but a small distance between the solubility of the combinations that may be formed. The proportions of the substances, in quantity, then determine the formation of the different salts, either by a first crystallization, or by crystallizations which, by subtracting some of the parts, must alter the proportion, and occasion a difference in the disposition of the remaining principles to crystallize in the successive formation of the salts. It is here that the application of the theory of the quiescent and divellent affinities may produce many errors, by leading us to conclude from the commencement of the phenomenon that the subsequent effects will be all of the same description, though in reality a succession of opposite combinations may be established, according to the forces which are made to act at the moment of their respective separations.

The joint consideration of the difference of solubility, and of the proportions employed, or which vary at the different periods of an operation, must consequently be our sole guide in the explanation of the successive formation of different salts which have no great difference in solubility. It is true, nevertheless, that the mutual action of the substances themselves may produce some difference in the results, which would be indicated by the preceding observations.

All substances which are in solution exert a mutual action, which increases their solubility. Hence the reason why it is difficult to obtain, by a first crystallization, each salt in a state of purity; except in the case where it differs considerably from the others in its force of crystallization. Hence arise the uncrystallizable residues which succeed the crystallizations wherein salts are found in the liquid, which possess but little force of cohesion. But here also the consideration of the proportions and of the solubility will still be useful to predict the existence and composition of an uncrystallizable residuum.

While the substances are in solution, the action which they mutually exert renders it easy to expel an acid from a combination, though, according to the received opinion, it ought to assume the place of another, and which was supposed to be weaker.

When insolubility is considered, it must not be regarded as an absolute property, but as a property relative to the liquid in which a precipitation is made. Thus a combination insoluble in water may lose this property when the water holds alkali in solution.

In all the experiments I have described, and in several others which I have thought it useless to describe, I perceived no change of saturation, either after the mixture of the neutral salts, or after the separation of the precipitates or crystallizations which took place, except in the experiments P, Q, made with a metallic substance. This permanent state of neutralization, after the change of bases which took place, seems to indicate that the acids have constant relations of quantity, in the neutral salts they form, with different alkaline or earthy bases. So that if the sulphuric acid for example, be found in greater proportion in the sulphate of pot-ash than in the sulphate of lime; the muriatic acid with which it may make an exchange of base will be found in the same ratio of quantity in the muriate of lime and in the muriate of pot-ash; a conclusion that would not agree with the proportions which have often been attributed by chemists to the component parts of the different non-metallic salts. Guyton has already made several very just and important reflexions on this subject, and he quotes the observations of Richeter, with whose work I am not yet acquainted. (*Annales de Chimie*, tom. xxv. p. 292).

(*To be continued.*)

The reader will perceive that I have supposed this treatise to have been concluded in a former number; into which error I was led by the want of any intimation in the *Annals* that more was to be expected. But every cultivator of science will rejoice with me that these researches, which shew the wonderful skill and acuteness of their author, and must produce an almost total revolution in our observations and reasoning on chemical effects, are more extensive than we at first imagined.—N.

VII.

On the Use of Steam as a Vehicle for conveying Heat from one Place to another.

By COUNT RUMFORD.*

MORE than fifty years ago, Colonel William Cook, in a Paper presented by him to the Royal Society, and published in their Transactions, made a proposal for warming rooms by means of metallic tubes filled with steam, and communicating with a boiler situated out of the room; which proposal was accompanied by an engraving, which showed, in a manner perfectly clear and distinct, how this might be effected. Since that time this scheme has frequently been put in practice with success, both in this country and on the Continent. † Many attempts have likewise been made, at different periods, to heat liquids by means of steam introduced into them; but most of these have failed: and, indeed, until it was known that fluids are nonconductors of heat, and, consequently, that heat cannot be made to *descend* in them—(which is a recent discovery),—these attempts could hardly succeed; for, in order to their being successful, it is absolutely necessary that the tube which conveys the hot steam should open into the *lowest part* of the vessel which contains the liquid to be heated, or on a level with its bottom;—but as long as the erroneous opinion obtained, that heat could pass in fluids *in all directions*, there did not appear to be any reason for placing the opening of the steam tube *at the bottom of the vessel*, while many were at hand which pointed out other places as being more convenient for it.

But to succeed in heating liquids by steam, it is necessary, not only that the steam should enter the liquid at the bottom of the vessel which contains it, but also that it should enter *it coming from above*. The steam tube should be in a vertical position, and the steam should *descend* through it previous to its entering the vessel, and mixing with the liquid which it is to heat; otherwise this liquid will be in danger of being forced back by this opening into the steam boiler; for the hot steam being suddenly condensed on coming into contact with the cold liquid, a vacuum will necessarily be formed in the end of the tube; into which vacuum the liquid in the vessel—pressed by the whole weight of the incumbent atmosphere—will rush with great force, and with a loud noise; but if this tube be placed in a vertical position, and if it be made to rise to the height of six or seven feet, the liquid which is thus forced into its lower end will not have time to rise to that height before it will be met by steam and obliged to return back into the vessel.—There will be no difficulty in arranging the apparatus in such a manner as effectually to prevent the liquid to be heated from

* Journal of the Royal Institution I. 34.

† Although one should naturally imagine that the notoriety of these facts would have been sufficient to prevent all attempts in our days to claim a right to this invention, yet it is said that a patent for it was taken out only a few years ago.

being forced backwards into the steam boiler; and, when this is done, and some other necessary precautions to prevent accidents are taken, steam may be employed with great advantage for heating liquids; and for keeping them hot, in a variety of cases, in which fire, applied immediately to the bottoms of the containing vessels, is now used.

In dyeing, for instance, and in brewing; and in the processes of many other arts and manufactures, the adoption of this method of applying heat would be attended, not only with a great saving of labour and of fuel; but also of a considerable saving of expence in the purchase and repairs of boilers, and of other expensive machinery: for when steam is used instead of fire for heating their contents, boilers may be made extremely thin and light; and, as they may easily be supported and strengthened by hoops and braces of iron, and other cheap materials, they will cost but little, and seldom stand in need of repairs. To these advantages we may add others of still greater importance: boilers intended to be heated in this manner may, without the smallest difficulty, be placed in any part of a room—at any distance from the fire;—and in situations in which they may be approached freely on every side. They may moreover easily be so surrounded with wood, or with other cheap substances which form warm covering, as most completely to confine the heat within them, and prevent its escape. The tubes by which the steam is brought from the principal boiler (which tubes may conveniently be suspended just below the ceiling of the room) may, in like manner, be covered, so as almost intirely to prevent all loss of heat by the surfaces of them; and this to whatever distances they may be made to extend.

In suspending these steam tubes, care must, however, be taken to lay them in a situation *not perfectly horizontal* under the ceiling, but to incline them at a small angle, making them rise gradually from their junction with the top of a large vertical steam tube, connecting them with the steam boiler, quite to their farthest extremities: for, when these tubes are so placed, it is evident that all the water formed in them, in consequence of the condensation of the steam in its passage through them, will run backwards and fall into the boiler, instead of accumulating in them, and obstructing the passage of the steam,—which it would not fail to do were there any considerable bends or wavings, upwards and downwards, in these tubes—or of running forward and descending with the steam into the vessels containing the liquids to be heated;—which would happen if these tubes inclined *downwards*, instead of inclining upwards, as they recede from the boiler.

(To be continued.)

*Mr. Cuthbertson's Experiments on the
Oxidation of Metals by Electricity.*

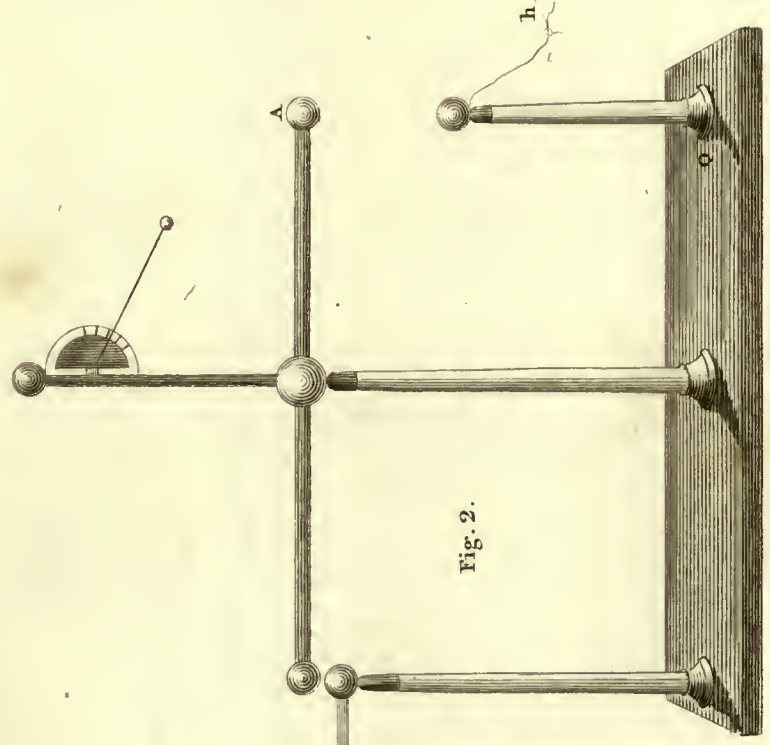
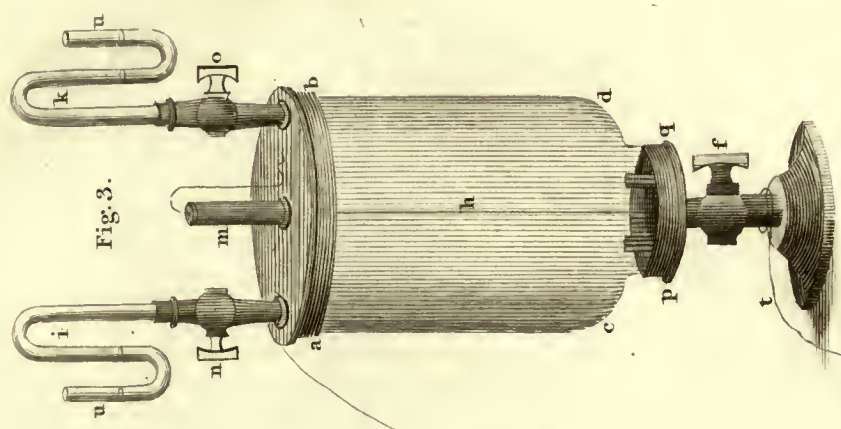


Fig. 1.

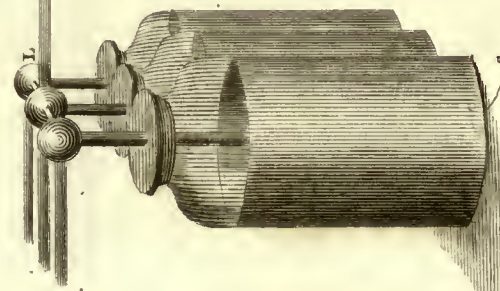


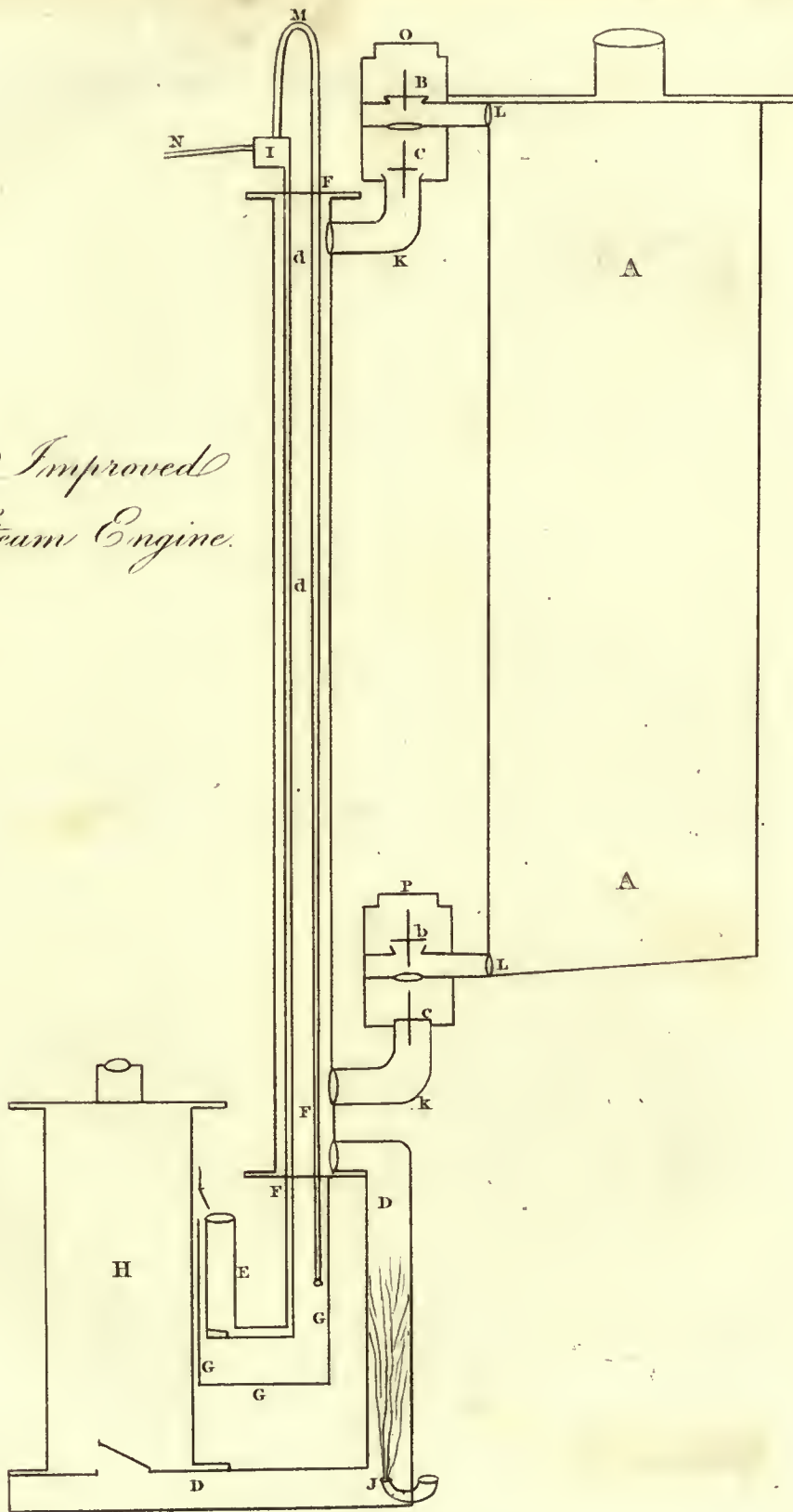
Fig. 2.

Fig. 4.





*Improved
Steam Engine.*





A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

AUGUST, 1801.

ARTICLE I.

A Letter from THOMAS YOUNG, M. D. F. R. S. Professor of Natural Philosophy in the Royal Institution, respecting Sound and Light, and in Reply to some Observations of Professor ROBISON.

TO MR. NICHOLSON.

SIR,

IN the supplement of the Encyclopædia Britannica, are inserted several excellent articles by Professor Robison of Edinburgh: one of them appears to require some public notice on my part, and I consider your valuable Journal as the most eligible channel for such a communication, especially as you have lately done me the honour of reprinting the paper which gave rise to the Professor's animadversions. But in the first place, I shall beg leave to recall the attention of your readers, by a summary enumeration, to the principal positions which I have in that paper endeavoured to establish.

I. Sound, as transmitted through the atmosphere, consists in an undulatory motion of the particles of the air, Sect. III. This is generally admitted; but as the contrary has even very lately been asserted, it is not superfluous to have decisive evidence of the fact. Professor Robison's experiment with the stop-cock furnishes an argument nearly similar.

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Y

2. A

2. A current of air, forced by a moderate pressure through a cylindrical pipe, diverges the less as its velocity is less, Sect. II.

3. At a certain point the divergency of such a current increases suddenly, and the current mixes with the surrounding air, Sect. II.

4. So far is such a motion from spreading equally in all directions, that on every side of the current the air is urged more towards it than from it, Sect. II.

5. Sound, admitted through an aperture, does not by any means diverge equally in all directions, and is probably very weak except in directions nearly rectilinear. From position 2 and 4, and from experience, Sect. VI.

6. Sound probably decays in the duplicate ratio of the distance, Sect. VII.

7. A similar blast of air produces nearly a similar sound, in organ pipes properly commensurate, Sect. VIII.

8. Light is probably the undulation of an elastic medium, Sect. X.

A. Because its velocity in the same medium is always equal.

B. Because all refractions are attended with a partial reflection.

C. Because there is no reason to expect that such a vibration should diverge equally in all directions, and because it is probable that it does diverge in a small degree in every direction.

D. Because the dispersion of differently coloured rays is no more incompatible with this system than with the common opinion, which only assigns for it the nominal cause of different elective attractions.

E. Because refraction and reflection in general are equally explicable on both suppositions.

F. Because inflection is as well, and, it may be added, even much better explained by this theory.

G. Because all the phenomena of the colours of thin plates, which are in reality totally unintelligible on the common hypothesis, admit a very complete and simple explanation by this supposition. The analogy which is here superficially indicated will probably soon be made public more in detail; and will also be extended to the colours of thick plates, and to the fringes produced by inflection, affording, from Newton's own elaborate experiments, a most convincing argument in favour of this system.

9. The particles of air may be jointly actuated by two or more sounds, and in this case, the several motions are to be added or subtracted, in order to find the actual joint motion, Sect. XI.

10. The grave harmonic produced by a major third is accompanied by a very audible twelfth. This circumstance is explained, and the effect of subordinate notes and subaltern stops, on the quality of sounds, is shown by figures, Sect. XI.

11. A noise returning every second, if audible, would be a C. From Sauveur; with an experiment, Sect. XII.

12. A chord retains always the form of its initial vibration. From experiments, in favour of Euler's theorem, against the simple harmonic curve, Sect. XIII.

13. The

13. The vibration of a chord is scarcely ever performed in the same plane. Its revolutions, and its subordinate vibrations, may be rendered distinctly visible under the microscope, Sect. XIII.

14. If a chord be inflected at any point of aliquot division, the harmonic secondary note corresponding to that division will not be audible; an experiment contradictory to some theories of the origin and of the inseparable nature of harmonic sounds, Sect. XIII.

15. The human voice is analogous to the organ pipe denominated from it, which consists of a tongue piece without any commensurate tube: and the falsetto is probably formed by the upper orifice of the trachea assuming the functions of the glottis, Sect. XV.

16. A temperament of progressive imperfection is the most convenient for practical music, and is easily approximated by tuning six perfect, and six equally imperfect fifths, Sect. XVI.

From the detached nature of the subjects which I have here enumerated, and the imperfect state of these branches of the mathematics to which they refer, it would have been in vain to attempt a very perspicuous and detailed discussion of them. My researches on these subjects have been much interrupted, and probably will not be very shortly resumed; but if they are of no further use to any person, I shall not think my labour lost; for I flatter myself that the inferences, which they have led me to draw respecting the theory of colours, will throw new light on all the most interesting parts of optics, while by a comparison with the obvious inferences from Dr. Herschel's important discoveries, they will also lead to some material illustrations of the phenomena of heat.

I shall now trouble you with some remarks in reply to Professor Robison: the passage to which I allude is this:

"We are surprised to see this work of Dr. Smith greatly undervalued, by a most ingenious gentleman in the Philosophical Transactions for 1800, and called a large and obscure volume, which leaves the matter just as it was, and its results useless and impracticable. We are sorry to see this; because we have great expectations from the future labours of this gentleman in the field of harmonics, and his late work is rich in refined and valuable matter. We presume humbly to recommend to him attention to his own admonitions to a very young and ingenious gentleman, who, he thinks, proceeded too far in animadverting on the writings of Newton, Barrow, and other eminent mathematicians." *Encyclop. Brit. Suppl. Art. Temperament*, p. 652.

According therefore to the author of this article, I have in the first place taken the liberty of giving severe advice, to a young mathematician who had never asked it; secondly, this advice is equally applicable to my own presumption; and thirdly, Dr. Smith's treatise on harmonics is a work intitled to the highest praise.

I did in fact endeavour to shew, that the gentleman in question had overlooked the labours of some former authors relative to his subject, but I accompanied my remarks with nothing like admonition. After observing that at present a geometrician may very easily fancy he has made discoveries, when the same facts were known and forgotten long before he existed, I proceeded in these words:

"An instance of this has lately occurred to a young gentleman in Edinburgh, a man who certainly promises in the course of time to add considerably to our knowledge of the laws of nature. The tractory—was first described by Huygens: Bemie and Perks have shewn, that it is the involute of the catenaria.—The curve which the author calls a cycloid is the companion of a trochoid, and is only a distortion of the figure by which Newton had very simply and elegantly solved the same problem.—On the whole it appears, that this ingenious gentleman has been somewhat unfortunate in the choice of those problems, which he has selected as specimens of the elegance of the modern mode of demonstration." Essay on cycloidal curves, British magazine for April 1800. Is this an admonition? Or is there here any remark which can at all apply to my insensibility to Dr. Smith's merit in harmonics?

I have read Dr. Smith's work with attention, and I imagine, from the polite manner in which Professor Robison is pleased to speak of my essay, he will not hesitate to allow that I have understood it. I took it up with great expectations; these expectations having been completely disappointed, I thought it right to state my cool and unprejudiced opinion of its merits, in order to prevent a similar disappointment in others. It is impossible therefore that an "attention" to any "admonitions" of a general nature, wherever they may be found, can influence such an opinion; and so far only as I am supposed to be an incompetent judge on the subject of harmonics, can it be asserted that it was either blameable or superfluous for me to express that opinion. As a mathematician and an optician I value Dr. Smith highly, but I must still beg leave to affirm, that his whole book of harmonics contains far, far less information, than either of the articles Temperament and Trumpet in the supplement of the Encyclopædia.

I do not mean to be understood that this work is so contemptible, as not to contain the least particle of important matter; but it appears to me that its errors counterbalance its merits. The only improvement on which Professor Robison himself seems to set a high value, is the application of the phenomena of beats to tuning an instrument: on the other hand, I conceive that the mistatement relative to the non-interference of different sounds, is an inaccuracy which far outweighs the merit of Dr. Smith's share of that improvement. I have asserted, that "Dr. Smith has written a large and obscure volume, which for every purpose but for the use of an impracticable instrument, leaves the whole subject of temperament precisely where it found it," and that "the system proposed for his changeable harpsichord is neither in that nor in any other form capable of practical application." Professor Robison on the contrary says, "we do not see how it can be disputed, that Dr. Smith's theory of the beating of imperfect consonances, is one of the most important discoveries both for the practice and the science of music, that have been offered to the public. We are inclined to consider it as the most important that has been made since the days of Galileo. We are obliged to call it his discovery. Mercennus indeed had taken particular notice of this undulation of imperfect consonances, and had offered conjectures as to their causes; conjectures not unworthy of his great ingenuity. Mr. Sauveur also takes a still more particular notice of this phenomenon, and makes a most ingenious

"use

"use of it for the solution of a very important musical problem." P. 652 and 651. Why then are we obliged to call it Dr. Smith's discovery, or indeed any discovery at all? Sauveur had already given directions for tuning an organ pipe by means of the rapidity of its beating with others. *Mem. de l'Ac.* 1701. p. 475. Ed. Amst. Dr. Smith ingeniously enough extended the method: but it appears to me that the extension was perfectly obvious, and wholly undeserving of the name either of a discovery or of a theory. If Professor Robison thinks otherwise, there is nothing further to be said; but in all probability Dr. Smith considered this improvement as a very inconsiderable part of the merit of his treatise. No doubt an organ may be more accurately tuned by counting the beats than by any other method, although it may be questioned, whether the advantage of counting the absolute frequency of the beats will ever practically compensate the tediousness of the process.

It remains to be considered, whether Dr. Smith's changeable harpsichord is or is not an impracticable instrument; for, whatever Signor Doria might exclaim, Dr. Smith himself does not recommend his scale for common use. It is the opinion of many unprejudiced practical persons, that all occasional introductions of different semi-tones is perfectly impracticable; and some who have heard the effect of Dr. Smith's instrument, have declared that to them it was by no means agreeable. And indeed if we pay sufficient attention to the passages and modulations of the greatest composers, we shall be convinced, that granting all possible dexterity in the performer, it would be absolutely impracticable to adapt them to an instrument, so different from that for which they were composed, as Dr. Smith's is from the common harpsichord. It may easily be conceived, that an organ very correctly tuned, as Mr. Watts's probably was, for a particular key, might appear "*sopra modo bellissimo*" in that key; but the sequel of the story shews literally what Dr. Smith has allowed, that his temperament is inapplicable to our instruments, since it was utterly impossible to sing with it in the key of E e s, a key of exceedingly frequent occurrence. I have been informed on the best authority, that Dr. Smith restricted the organist of Trinity College to such keys and modulations, as were best suited to the system by which the organ was tuned; and that organ, as well as the instruments which were made for Dr. Smith, has long been tuned according to the more common method.

I spoke of Dr. Smith's system with flattened major thirds as of no value, not with regard to its intrinsic merits, but because it was not intended for any instrument in common use; since in these instruments the difficulty is not so much how to divide the imperfection among the thirds and fifths of the same scale, as to proportion properly the imperfections of the thirds of different keys. Yet I do not mean it to be understood, that I can agree to the solidity of those foundations on which Dr. Smith has built his system for a single scale: although to Stanley and to Doria it might be pleasing, because its imperfections are far too small to offend the ear. Professor Robison justly observes, that different persons differ exceedingly in their estimation of the effect of the same temperament on different concords, and that much of this arises from their different dispositions: it appears therefore that Dr. Smith.

Smith was too precipitate in laying down his principle for the comparison of the effects of temperament.

With respect to the system which I have proposed, Professor Robison thinks that the temperaments of several of the thirds which occur frequently are much too great. If we wish to form a judgment of any system of temperament, it must be by comparison with some other. It does not appear with what system Professor Robison would wish the comparison to be made, but he rather seems to incline to the equal temperament, although he gives directions for tuning by another. At any rate, no temperament of an interval can be said to be much too great, unless it be greater than that of the same interval in the system of equal temperament; for if any interval be made more perfect than this, some other similar interval must be as much less perfect. In my system, the only thirds perceptibly greater than those of the equal temperament, are the major thirds on E, A e s, B, C i s, and F i s, and the minor on C, C i s, F, G i s, B e s, and E e s. Of these none can be said to occur frequently, except the major third on E, and the minor on C. The sixths require no separate consideration. Now since the minor chord is intended to be less completely harmonious than the major, its character will be by no means materially impaired by this imperfection, which it would be somewhat difficult to remove. The third on E is not sharp enough to be very offensive, but in compliance with the usual practice of making this third somewhat more perfect than the interval of A e s and C, I have, in the method recommended for common use, made it a better third than that of the equal temperament. The directions given for tuning in § 68, and in § 80, of the article, are liable to far greater objections. For instance, the temperament of the III ds on A e s and F i s, in the latter, is about .00880, or more than a comma and a half; which Professor Robison will readily allow to be "much too great" for any thirds; since he has asserted, with Dr. Smith and others, that the error of a comma would be intolerable. Mr. Maxwell has, however, very decidedly proved, in his Essay on Tune, that the greatest harmonists, Corelli, Tartini, and Giardini, have admitted very frequently the error of a comma in their most refined compositions. And I have the authority of several celebrated performers on stringed and wind instruments for asserting, that they take of choice the characteristic *femi-tone*, leading into the key note, considerably sharper than the same note is tuned on any keyed instruments, making an imperfection of nearly two commas in the relation as third of the dominant, which is the fundamental note of the chord: while in the mean time our theorists have been labouring, by the most complicated contrivances, to introduce notes into keyed instruments, which shall have exactly a contrary effect, by making the ascending *femi-tone* as wide a step as possible. On asking very lately the opinion of a practical musician of great eminence, and one who in every respect does honour to his profession, he decidedly agreed in the superiority of such a diminished *femi-tone*, and observed that the key of E derived a very elegant character from the usual method of tuning D i s as E e s, a minor third to C: hence, the III ds E e s and G being very little tempered, the III d on the dominant B must be

be about a comma and a half too sharp. The fact is, that in this case the harmony is somewhat impaired in order to improve the melody. The interval of 15 to 16 is far too small to be distinctly conceived as commensurate, it possesses therefore no melody in virtue of the perfection of its ratio, and a certain elegance of expression is added by approaching to the natural and colloquial ascent of a voice by imperceptible degrees. It must, however, be confessed, that some excellent musicians prefer a purer harmony, and in this, as in all other matters of taste, considerable latitude must be allowed for the habits and predilections of individuals.

I am, SIR,

With great respect,

Your obedient humble servant,

THOMAS YOUNG.

No. 48, Welbeck-Street,, July 13, 1801.

ERRATA IN THE PAPER ON SOUND AND LIGHT.

In Table XII, for E b 83810 read E b 84197; for .0011562 read .0010116. In Fig. 53 the E b (Q) is too near D, and the E b (Y) should be above instead of below it.

II.

On certain Points of Nomenclature. By a Correspondent.

To Mr. NICHOLSON.

SIR,

London, July 15, 1801.

THE gas at this time, whose effects on breathing it are an object of great curiosity among chemists and amateurs, denominated by its discoverer Dr. Priestley, *dephlogisticated nitrous gas*, and subsequently by the Dutch philosophers who investigated many of its properties, called *oxide of azote gas*, or *gaseous oxide of azote*, is now universally spoken of by the name *gaseous oxide*. The very ingenious and promising philosopher, and expert experimenter, whom the managers of the Royal Institution have engaged, has detected the mistake of Deiman, Troostwyk &c. who found out that it was capable of supporting *inflammation*, but erroneously concluded, that it could not possess *respiration*. Mr. Davy thereupon very justly enjoys the honor of the discovery of this, and other new properties of this oxide; but my object now is only to observe, that the term *gaseous oxide* carries no useful and discriminating import, but what is worse than no import at all, it is very liable to produce mistake and confusion; as was the case with the terms *fixed air*, *aerial acid*, &c. formerly

formerly used. It appears from the analysis of Deiman and Davy, that the oxide under observation consists of above 0,37 of oxygen, and 0,63 of azote or nitrogen. Secondly, That *nitrous gas*, or *nitrous oxide* consists of about 0,68 of oxygen, and 0,32 of azote. Accordingly Deiman distinguishes these two oxides by the names, *oxide of azote*, or of *nitrogen*, and *nitrous oxide*, which terms are little else but an inversion mutually of their order, and besides have no specific import. Is it worth while to propose the appellations, *sub-oxide of nitrogen*, or *azote*, and *oxide of nitrogen*, or *azote*, in place of the names in use? This method is analogous to the mode of the new nomenclature in its latest improvements, as in the instances *sub-muriate*, and *muriate of mercury*, *sub-carbonate*, and *carbonate of alkali*, *sub-phosphate*, and *phosphate of lime*, &c.

I am,

YOUR FRIEND.

HI.

On the Use of Steam as a Vehicle for conveying Heat from one Place to another.

By COUNT RUMFORD.

(Continued from page 160.)

IN order that clear and distinct ideas may be formed of the various parts of this apparatus,—even without figures,—I shall distinguish each part by a specific name: the vessel in which water is boiled in order to generate steam—and which, in its construction, may be made to resemble the boiler of a steam engine—I shall call the *steam boiler*:—The vertical tube, which, rising up from the top of the boiler, conveys the steam into the tubes (nearly horizontal), which are suspended from the ceiling of the room, I shall call the *prime conductor*: to the horizontal tubes I shall give the name of *horizontal conductors*, or simply, *conductors of steam*: and to the (smaller) tubes, which, descending perpendicularly from these *horizontal conductors*, convey the steam to the liquids which are to be heated, I shall, exclusively, appropriate the appellation of *steam tubes*.

The vessels in which the liquids are put that are to be heated I shall call the *containing vessels*.—These vessels may be made of any form; and, in many cases, they may, without any inconvenience, be constructed of wood, or of other cheap materials, instead of being made of costly metals, by which means a very heavy expence may be avoided.

Each *steam tube* must descend perpendicularly from the *horizontal conductor* with which it is connected, to the level of the bottom of the *containing vessel* to which it belongs; and, moreover, must be furnished with a good brass cock, perfectly steam-tight; which may best be placed at the height of about six feet above the level of the floor of the room.

This

This *steam tube* may either descend *within the vessel* to which it belongs, or *on the outside of it*, as shall be found most convenient. If it comes down on the outside of the vessel, it must enter it at its bottom, by a short horizontal bend; and its junction with the bottom of the vessel must be well secured, to prevent leakage. If it comes down into the vessel, on the inside of it, it must descend to the bottom of it, or at least to within a very few inches of the bottom of it; otherwise the liquid in the vessel will not be uniformly and equally heated.

When the steam tube is brought down on the inside of the containing vessel, it may either come down perpendicularly, and without touching the sides of it, or it may come down on one side of the vessel, and in contact with it.

When several steam tubes, belonging to different containing vessels, are connected with one and the same horizontal steam conductor, the upper end of each of these tubes, instead of being simply attached by folding to the under side of the conductor, must enter, at least one inch, within the cavity of it; otherwise the water resulting from a condensation of a part of the steam in the conductor, by the cold air which surrounds it, instead of finding its way back into the steam boiler, will descend through the steam tubes and mix with the liquids in the vessels below; but when the open ends of these tubes project upwards within the steam conductor, though it be but to a small height above the level of its under side, it is evident that this accident cannot happen.

It is not necessary to observe here, that, in order that the ends of the steam tubes may project within the *horizontal conductor*, the diameters of the former must be considerably less than the diameter of the latter.

To prevent the loss of heat arising from the cooling of the different tubes through which the steam must pass in coming from the boiler, all those tubes should be well defended from the cold air of the atmosphere, by means of warm covering; but this may easily be done, and at a very trifling expence. The horizontal conductors may be enclosed within square wooden tubes, and surrounded on every side by charcoal dust,—fine sawdust,—or even by wool; and the steam tubes, and prime conductor, may be surrounded first by three or four coatings of strong paper, firmly attached to them by paste or glue, and covered with a coating of varnish, and then by a covering of thick coarse cloth. It will likewise be advisable to cover the horizontal conductors with several coatings of paper, for if the paper be put on to them while it is wet with the paste or glue; and if care be taken to put it on in long slips or bands, wound regularly round the tube in a spiral line, from one end of it to the other, this covering will be useful, not only by confining more effectually the heat, but also by adding very much to the strength of the tube, and rendering it unnecessary to employ thick and strong sheets of metal in the construction of it.

However extraordinary and incredible it may appear, I can assert it as a fact, which I have proved by repeated experiments, that if a hollow tube, constructed of sheet copper $\frac{1}{20}$ of an inch in thickness, be covered by a coating of only twice as thick, or $\frac{1}{10}$ of an inch in thickness, formed of layers of strong paper, firmly attached to it by good glue, the

strength of the tube will be *more than doubled* by this covering. I found by experiments, the most unexceptionable and decisive, of which I intend at some future period to give to the public a full and detailed account, that the strength of paper is such, when several sheets of it are firmly attached together with glue, that a solid cylinder of this substance, the transverse section of which should amount to only one superficial inch, would sustain a weight of 30,000 lbs. avoirdupois, or above 13 tons, suspended to it, without being pulled asunder or broken. The strength of hemp is still much greater, when it is pulled equally, in the direction of the length of its fibres. I found, from the results of my experiments with this substance, that a cylinder of the size above mentioned, composed of the straight fibres of hemp, glued together, would sustain 92000 lbs. without being pulled asunder.

A cylinder, of equal dimensions, composed of the strongest iron I could ever meet with, would not sustain more than 66000 lbs. weight; and the iron must be very good not to be pulled asunder with a weight equal to 55000 lbs. avoirdupois.

I shall not, in this place, enlarge on the many advantages that may be derived from a knowledge of these curious facts. I have mentioned them now in order that they may be known to the public; and that ingenious men, who have leisure for these researches, may be induced to turn their attention to a subject, not only very interesting, on many accounts, but which promises to lead to most important improvements in mechanics.

I cannot return from this digression without just mentioning one or two results of my experimental investigations relative to the force of cohesion, or strength of bodies, which, certainly, are well calculated to excite the curiosity of men of science.

The strength of bodies of different sizes, *similar in form*, and composed of the *same substance*, or the forces by which they resist being pulled asunder by weights, suspended to them, and acting in the direction of their lengths,—*are not in the simple ratio of the areas of their transverse sections*, or of their *fractures*; but in a higher ratio; and this ratio is different in different substances.

The *form* of a body has a considerable influence on its strength, *even when it is pulled in the direction of its length*.

All bodies, even the most brittle, appear to be *torn asunder*, or their particles separated, or fibres broken, *one after the other*; and hence it is evident, that that *form* must be most favourable to the strength of any given body, pulled in the direction of its length, which enables the greatest number of its particles, or longitudinal fibres, to be separated to the greatest possible distance—short of that at which the force of cohesion is overcome,—before any of them have been forced beyond that limit.

It is more than probable that the apparent strength of different substances depends much more on the number of their particles that come into action before any of them are forced beyond the limit of the attraction of cohesion, than on any specific difference in the intensity of that force in those substances.

But to return to the subject more immediately under consideration.—As it is essential that the steam employed in heating liquids, in the manner before described, should enter

the containing vessel at, or very near, its bottom, it is evident that this steam must be sufficiently strong, or elastic, to overcome, not only the pressure of the atmosphere, but also the additional pressure of the superincumbent liquid in the vessel; the steam boiler must, therefore, be made strong enough to confine the steam, when its elasticity is so much increased by means of additional heat, as to enable it to overcome that resistance. This increase of the elastic force of the steam need not, however, in any case, exceed a pressure of five or six pounds upon a square inch of the boiler, or *one third part*, or *one half*, of an atmosphere.

It is not necessary for me to observe here, that in this, and also in all other cases, where steam is used as a vehicle for conveying heat from one place to another, it is indispensably necessary to provide *safety valves* of two kinds;—the one for letting a part of the steam escape, when, on the fire being suddenly increased, the steam becomes so strong as to expose the boiler to the danger of being burst by it;—the other for admitting air into the boiler, when, in consequence of the diminution of the heat, the steam in the boiler is condensed, and a vacuum is formed in it; and when, without this valve, there would be danger, either of having the sides of the boiler crushed, and forced inwards by the pressure of the atmosphere from without; or of having the liquid in the containing vessels forced upwards into the horizontal steam conductors, and from thence into the steam boiler. This last mentioned accident, however, cannot happen, unless the cocks in some of the steam tubes happen to be open.—The two valves effectually prevent all accidents.

The reader will, no doubt, be more disposed to pay attention to what has here been advanced, on this interesting subject, when he is informed that the proposed scheme has already been executed on a very large scale, and with complete success; and that the above details are little more than exact descriptions of what actually exists.

A great mercantile and manufacturing house at Leeds, that of Messrs. Gott, and Co. had the courage, notwithstanding the mortifying prediction of all their neighbours, and the ridicule with which the scheme was attempted to be treated, to erect a *dyeing house*, on a very large scale indeed, on the principles here described and recommended.

On my visit to Leeds the last summer, I waited on Mr. Gott, who was then mayor of the town, and who received me with great politeness, and shewed me the cloth halls, and other curiosities of the place; but nothing he shewed me interested me half so much as his own truly noble manufactory of superfine woollen cloths. I had seen few manufactories so extensive, and none so complete in all its parts. It was burnt to the ground the year before I saw it, and had just been rebuilt, on a larger scale; and with great improvements in almost every one of its details. The reader may easily conceive that I felt no small degree of satisfaction on going into the dyeing house, to find it fitted up on principles which I had had some share in bringing into repute, and which Mr. Gott told me he had adopted in consequence of the information he had acquired in the perusal of my *seventh* essay. He assured me that the experiment had answered, even far beyond his most sanguine expectations; and, as a strong proof of the utility of the plan, he told me, that his next door

neighbour, who is a dyer by profession, and who, at first, was strongly prejudiced against these innovations, has lately adopted them, and is now convinced that they are real improvements. Mr. Gott assured me that he had no doubt but that they would be adopted by every dyer in Great Britain in the course of a very few years.

The dyeing house of Messrs. Gott and Co. which is situated on the ground floor of the principal building of the manufactory, is very spacious, and contains a great number of coppers of different sizes; and as these vessels, some of which are very large, are distributed about promiscuously, and apparently without any order in their arrangement, in two spacious rooms, each copper appearing to be insulated, and to have no connection whatever with the others, all of them together form a very singular appearance. The rooms are paved with flat stones, and the brims of all the coppers, great and small, are placed at the same height, about three feet, above the pavement: some of these coppers contain upwards of 1800 gallons; and they are all heated by steam from one steam boiler, which is situated in a corner of one of the rooms.

The horizontal tubes which serve to conduct the steam from the boiler to the coppers are suspended just below the ceilings of the rooms: they are made, some of lead, and some of cast iron; and are from four to five inches in diameter; but when I saw them, they were naked, or without any covering to confine the heat. On my observing to Mr. Gott that coverings for them would be useful, he told me that it was intended that they should be covered, and that coverings would be provided for them.

The vertical *steam tubes*, by which the steam passes down from the horizontal *steam conductors* into the coppers, are all constructed of lead, and are from $\frac{3}{4}$ of an inch to $2\frac{1}{2}$ inches in diameter; being made larger or smaller according to the sizes of the coppers to which they belong. These steam tubes all pass down on the *outsides* of their coppers; and enter them horizontally at the level of their bottoms. Each copper is furnished with a brass cock, for letting off its contents; and it is filled with water from a cistern at a distance, which is brought to it by a leaden pipe. The coppers are all surrounded by thin circular brick walls, which serve not only to support the coppers, but also to confine the heat.

The rapidity with which these coppers may be heated, by means of steam, is truly astonishing. Mr. Gott assured me that one of the largest of them, containing upwards of 1800 gallons, when filled with cold water from the cistern, requires no more than *half an hour* to heat it till it actually boils!—By the greatest fire that could be made under such a copper, with coals, it would hardly be possible to make it boil in less than hour.

It is easy to perceive that the *saving of time* which will result from the adoption of this new mode of applying heat will be very great;—and it is likewise evident that it may be increased, almost without limitation, merely by augmenting the diameter of the steam tube: Care must, however, be taken that the boiler be sufficiently large to furnish the quantities of steam required. The *saving of fuel* will also be very considerable: Mr. Gott informed me that, from the best calculation he had been able to make, it would amount to near two thirds of the quantity formerly expended, when each copper was heated by a separate fire.

But

But these savings are far from being the only advantages that will be derived from the introduction of these improvements in the management of heat: there is one, of great importance indeed, not yet mentioned, which alone would be sufficient to recommend the very general adoption of them. As the heat communicated by steam can never exceed the mean temperature of boiling water by more than a very few degrees, the substances exposed to it can never be injured by it. In many arts and manufactures this circumstance will be productive of great advantages, but in none will its utility be more apparent than in cookery; and especially in public kitchens, where great quantities of food are prepared in large boilers; for, when the heat is conveyed in this manner, all the labour now employed in stirring about the contents of those boilers, to prevent the victuals from being spoiled by burning to the bottoms of them, will be unnecessary; and the loss of heat occasioned by this stirring, prevented; and, instead of expensive coppers, or metallic boilers, which are difficult to be kept clean, and often stand in need of repairs, common wooden tubs may, with great advantage, be used as culinary vessels; and their contents may be heated by *portable fire-places*, by means of steam boilers attached to them.

As these portable fire-places and their steam boilers may, without the smallest inconvenience, be made of such weight, form, and dimensions, as to be easily transported from one place to another by two men, and be carried through a door-way of the common width, with this machinery, and the steam tubes belonging to it, and a few wooden tubs, a complete public kitchen, for supplying the poor and others, with soups; and also with puddings, vegetables, meat, and all other kinds of food prepared by *boiling*, might be established in half an hour, in any room, in which there is a chimney (by which the smoke from the portable fire-place can be carried off); and, when the room should be no longer wanted as a kitchen, it might, in a few minutes, be cleared of all this culinary apparatus, and made ready to be used for any other purpose.

This method of conveying heat is peculiarly well adapted for heating baths: it is likewise highly probable that it would be found useful in the bleaching business, and in washing linen. It would also be very useful in all cases where it is required to keep any liquid at about the boiling point for a long time without making it boil; for the quantity of heat admitted may be very nicely regulated by means of the brass cock belonging to the steam tube. Mr. Gott shewed me a boiler in which shreds of skins were digesting in order to make glue, which was heated in this manner; and in which the heat was so regulated, that, although the liquid never actually boiled, it always appeared to be upon the very point of beginning to boil.

This temperature had been found to be best calculated for making good glue. Had any other *lower* temperature been found to answer better, it might have been kept up with the same ease, and with equal precision, by regulating properly the quantity of steam admitted.

I need not say how much this country is obliged to Mr. Gott, and his worthy colleagues. To the spirited exertions of such men, who abound in no other country, we owe one of the proudest distinctions of our national character; that of being an enlightened and an enterprising people.

IV.

*On the Identity of Galvanism and Electricity. By a Correspondent.**Edinburgh, June 20, 1801.*

To MR. NICHOLSON.

SIR,

I HOPE you will not consider me as one struggling against the conviction of truth, when I inform you, that I have repeated Mr. Cruickshank's experiment, of charging a vial with Galvanism successfully; but am inclined to draw a very different conclusion from his, in consequence of the phenomenon that presents itself, both during the performance of that experiment, and when it is enlarged and extended on the same principle.

The mode that I first used, was that of filling a common vial with salt water, which was plunged in another vessel also filled with salt water; though no great nicety was used, yet some attention was paid to the surfaces of the two liquors being nearly upon a level, and by that means it very well represented an electrical vial, coated to the same height within and without; then the inside was connected with one end of the pile, and the outside with the other. After different intervals of time, some longer, and some shorter, the longest of which extended to several hours, the vial was lifted out in one hand intirely from the vessel in which it stood, and the tongue applied to the wire connected with the inside; the constant result was, that whether it stood for a longer, or shorter period, a slight shock, or rather taste, was felt on the tongue. Expecting to increase this trifling sensation, by multiplying the number of the vials, I made a range of a dozen communicating all the insides, and all the outsides together by wire; after trying this mode by every means my imagination could suggest, I could perceive no increase of power whatever, but only the slight sensation, perceptible by means of one vial only.

The conclusion I am inclined to draw from the above facts is, that Galvanism cannot be accumulated by the same process with electricity, but that merely a small proportion of it can be communicated to fluids, and that consequently a marked distinction subsists between them, the Galvanic charge probably not being a fiftieth part of the power residing in the pile; whereas the electrical charge is oftener many hundred times beyond the power of the machine that imparts it; and that the former can be increased only, by increasing the original power.

I have also repeated my former experiments of charging an electrical vial through the insulated pile; and to determine the matter with the greatest accuracy, I made use of Lane's electrometer; when its knobs were set at a given distance, by means of the graduated wire, the discharge constantly took place at the same number of revolutions of the machine, which-ever wire was connected with the jar, provided the machine was in
equally.

equally good order, and the weather equally favourable; but whenever an increase or diminution was observed upon trying one wire, a similar change was perceived, upon changing it for the other.

I have also found, that plants cannot be altogether said to be non-conductors of Galvanism; by connecting one end of the pile with the stem of a plant, and taking the other wire in the hand. When the tongue was applied to any of the leaves, even at a considerable distance from the stem, a slight taste was perceived; but though I allowed the same plant to remain for above twelve hours with the two ends of the pile connected with different parts of it, it sustained no injury whatever. It is to be observed, that the pile was strong enough to give a very severe shock through several human bodies.

I think the result of the above must lead us to conclude, that though a small quantity of electricity may attend the phenomena of Galvanism, (and there is scarcely an operation in nature wherein a small quantity is not produced, and as you yourself have observed, that the very motions of the human body, and the drapery that covers it, will produce some:) yet that the principal phenomena of Galvanism, viz. the shock, the spark, and the snap, are not electrical, and we must here use the same mode of reasoning, that we would in chemistry, that when one of the leading properties of any body was absent, that it must have lost it, either by combination with another, or by decomposition; but as you are inclined to attribute the want of the effect of attracting light bodies to the want of velocity in Galvanism, I beg to be informed, whether Mr. Cruickshank's very powerful machine affects the electrometer equally with an electrical vial of the same strength, for I am at a loss what to call velocity, if the ellision of light from the air, producing a sound to be heard in another room, and rushing to a conductor at a distance, be not instances of velocity equal to any observed in electricity.

With the greatest respect, and thanks both to you and Mr. Cruickshanks, for the readiness with which you attended to my last, and the full explanation you took the trouble to honour me with,

I am, SIR,

A FRIEND TO THE EXPERIMENTAL RESEARCH OF TRUTH.

V.

An Account of a new Eudiometer. By Mr. DAVY. From the Journal of the Royal Institution.

THE dependance of the health and existence of animals upon a peculiar state of the atmosphere, and the relations of this state to processes connected with the most essential wants of life, have given interest and importance to inquiries concerning the composition and properties of atmospheric air.

This

This elastic fluid has been long known to consist chiefly of oxygen and nitrogen, mingled together, or in a state of loose combination, and holding in solution water.

A variety of processes have been instituted with the view of determining the relative proportions of the two gases, but most of them have involved sources of inaccuracy; and lately all, except two (the slow combustion of phosphorus, and the action of liquid sulphurets), have been generally abandoned.

Both phosphorus and solution of sulphuret of pot-ash absorb the whole of the oxygen of atmospheric air at common temperatures, and they do not materially alter the volume, or the properties of the residual nitrogen; but their operation is extremely slow; and in many cases it is difficult to ascertain the period at which the experiment is completed.

I have lately employed as an eudiometrical substance the solution of green muriate, or sulphate, of iron, impregnated with nitrous gas; and I have found that it is in some respects superior to many of the bodies heretofore used, as it rapidly condenses oxygen without acting upon nitrogen; and requires for its application only a very simple and a very portable apparatus.

This fluid is made by transmitting nitrous gas through green muriate, or sulphate, of iron, dissolved to saturation in water*. As the gas is absorbed, the solution becomes of a deep olive brown, and when the impregnation is completed it appears opaque and almost black. The process is apparently owing to a simple elective attraction; in no case is the gas decomposed; and under the exhausted receiver it assumes its elastic form, leaving the fluid with which it was combined unaltered in its properties.

The instruments necessary for ascertaining the composition of the atmosphere, by means of impregnated solutions, consist simply of a small graduated tube, having its capacity divided into one hundred parts, and greatest at the open end; and of a vessel for containing the fluid.

The tube, after being filled with the air to be examined, is introduced into the solution; and, that the action may be more rapid, gently moved from a perpendicular towards a horizontal position. Under these circumstances the air is rapidly diminished; and, in consequence of the dark colour of the fluid, it is easy to discover the quantity of absorption. In a few minutes the experiment is completed, and the whole of the oxygen condensed by the nitrous gas in the solution in the form of nitrous acid.

In all eudiometrical processes with impregnated solutions, the period at which the diminution is at a stand must be accurately observed; for, shortly after this period, the volume of the residual gas begins to be a little increased, and, after some hours, it will often fill a space greater by several of the hundred parts on the scale of the tube, than that which it occupied at the maximum of absorption.

This circumstance depends upon the slow decomposition of the nitrous acid (formed during the experiment), by the green oxide of iron, and the consequent production of a

* Dr. Priestley first observed this process: for a particular account of it, see *Researches, Chemical and Philosophical*, page 152. Johnson.

small quantity of aeriform fluid (chiefly nitrous gas) * ; which, having no affinity for the red muriate, or sulphate, of iron produced, is gradually evolved, and mingled with the residual nitrogen.

The impregnated solution with green muriate is more rapid in its operation than the solution with green sulphate. In cases when these salts cannot be obtained in a state of absolute purity, the common or mixed sulphate of iron may be employed. One cubic inch of moderately strong impregnated solution is capable of absorbing five or six cubic inches of oxygen, in common processes; but the same quantity must never be employed for more than one experiment.

A number of comparative experiments, made on the constitution of the atmosphere at the Hotwells, Bristol, in July, August, and September, 1800, with phosphorus, sulphurets of alkalies, and impregnated solution, demonstrated the accuracy of the processes in which the last substance was properly employed. The diminutions given by the sulphurets were indeed always greater by a minute quantity than those produced by phosphorus and impregnated solutions: but the reason of this will be obvious to those who have studied the subject of Eudiometry. In no instance was it found that 100 parts in volume of air contained more than 21 of oxygen: and the variations connected with different winds, and different states of temperature, moisture, &c. were too small, and too often related to accidental circumstances, to be accurately noticed.

In analysing the atmosphere in different places, by means of impregnated solutions, I have never been able to ascertain any notable difference in the proportions of its constituent parts. Air, collected on the sea at the mouth of the Severn, on October the 3d, 1800, which must have passed over much of the Atlantic, as the wind was blowing strong from the west, was found to contain 21 per cent. of oxygen in volume; and this was nearly the proportion in air sent from the coast of Guinea, to Dr. Beddoes, by two surgeons of Liverpool.

If we compare these results, with the results gained more than twenty years ago, by Mr. Cavendish, from experiments on the composition of atmospherical air, made at London and Kensington; considering, at the same time, the researches of Berthollet in Egypt and at Paris, and those of Marti in Spain, we shall find strong reasons for concluding, that the atmosphere, in all places exposed to the influence of the winds, contains very nearly the same proportions of oxygen and nitrogen, a circumstance of great importance; for, by teaching us that the different degrees of salubrity of air do not depend upon differences in the quantities of its principal constituent parts, it ought to induce us to institute researches concerning the different substances capable of being dissolved or suspended in air, which are noxious to the human constitution: particularly as an accurate knowledge of their nature and properties would probably enable us, in a great measure, to guard against, or destroy, their baneful effects.

* The decomposition of nitrous acid, by solutions containing oxide of iron, at its minimum of oxidation, is a very complex process. The green oxide, during its conversion into red oxide, not only decomposes the acid, but likewise acts upon the water of the solution; and ammoniac is sometimes formed, and small portions of nitrous oxide and nitrogen evolved with the nitrous gas.

V.

On the Discolouration of Silver by Bird's Eggs, &c. In a Letter from ANTHONY CARLISLE, Esq.

To MR. NICHOLSON.

SIR,

I HAVE been lately occupied in tracing the natural history of bird's eggs, for a paper which has been in part laid before the Linnæan Society. Among other needful inquiries, I had occasion to make some experiments on the properties of the Albumen, Vitellus, and Gas contained in the *folliculus æris* of eggs, which belong more properly to chemical history. As some of the results seem new, I beg leave to offer them for insertion in your Journal.

The well known fact of silver being discoloured by eggs, induced me to try which of the substances composing a fowl's egg is the cause of this appearance. I put the albumen, or white part of a new laid hen's egg, into a clean polished silver spoon; and the intire vitellus, or yolk, freed from its membrane, and all adhering albumen into another similar spoon. They were each exposed to a heat above that of boiling water, coagulated and reduced nearly to dryness, then left to cool, and twelve hours afterwards the silver remained untarnished in both.

A silver speculum placed in water, was adapted to receive the air contained in the *folliculus æris*, which remained exposed to this gas for two days, without any discolouration.

I mixed the albumen and vitellus together in a spoon, and exposed them to heat without discolouring the silver. The intire albumen of a new laid egg was put into ten ounces (by measure) of boiling distilled water in a glazed pipkin, and kept till it was coagulated firmly: a clean silver spoon had been immersed with the albumen, to which it adhered in several places; after setting the vessel in a cool place for an hour, the silver immersed had acquired a deep blue and brown colour, with various prismatic shades.

An intire vitellus, freed from all other substances, was put into boiling water, together with a silver spoon as in the last described experiment, but the silver did not receive any tarnish.

Ten ounces by measure of the water in which the albumen of one egg had been boiled was filtered, and put into a glass vessel with a silver spoon; after remaining there for two hours it had received a deep copper colour.

The same quantity of water in which the vitellus of one egg had been boiled, was put into a glass with a silver spoon, and remained six hours without producing any change of colour. The water in the two last experiments was at 60° of Fahrenheit.

The

The matter which produced the discolouration of the silver proved to be sulphurated hydrogen, and it is worthy of remark, that this compound is not formed by the albumen of eggs without the addition of water; the water contained in the albumen itself seeming to be insufficient to give the hydrogen required, or its combination with this substance not admitting of decomposition*.

The smell of sulphurated hydrogen appears much stronger in the water where albumen has been boiled, if it have had silver immersed in it: perhaps the silver gives the predisposing affinities. That so large a quantity of sulphur should be contained in the rudimental materials of a bird, is a curious fact: but as sulphur is a substance still imperfectly known, it is difficult to say whence it is derived, and for what purposes it is employed.

The serum of the human blood contains sulphur, which in some experiments made for me by Mr. Fred. Accum yielded sulphurated hydrogen abundantly.

The air contained in the folliculus aeris, appears from the few experiments which I have made to be analogous to atmospheric air.

I am, dear SIR,

Your much obliged, &c. &c.

Soho Square, July 4, 1801.

A. CARLISLE.

VI.

Inquiries relative to the Laws of Affinity. By CIT. BERTHOLLET. †

(Continued from page 158.)

Of the Metallic Solutions and Precipitates.

IN the two preceding Memoirs, I have principally considered those substances which are simple, and of which the composition is not variable; but it is known that the metallic oxides have different properties, according to the state of the oxidation; which must necessarily affect their chemical action.

The object of this memoir is to examine, what may be the influence of the degree of oxidation of the metals in their chemical action, and in general to compare this action with that of other substances, independantly of the properties with which I have been occupied (*Inquiries*, Art. XIII.)

* Perhaps the discolouring of silver spoons in eating boiled eggs, may arise from the water of the saliva being repeatedly brought into contact with the heated albumen, or from the water in which the eggs had been boiled having permeated the shell.

† *Annales de Chimie*, tom. XXXVIII. p. 113.

The solutions and precipitates of mercury have appeared to me to be particularly worthy of attention, because chemists have observed them with more care, and it is more easy to determine the conditions.

2. Fourcroy has described in the *Memoirs of the Academy of Sciences*, 1790, a sulphate of mercury, which he proves to be analogous to calomel, that is to say, formed by mercury little oxygenated. He has shewn that the sulphate of mercury, particularly when such a degree of heat only was employed as not to produce complete desiccation of the sulphuric acid and mercury, was divided by the action of the water into two sulphates, one of which may be called oxygenated sulphate, and the other mild sulphate. A simple method of obtaining this last combination, is to dilute by a nearly equal volume of water, the sulphuric acid which is treated with mercury, and to cause the mixture to boil; little sulphureous acid is formed, and instead of oxygenated sulphate of mercury, that sulphate is obtained of which the properties have been so well investigated by Fourcroy.

In the determination which Fourcroy has given of the constituent parts of the mild sulphate of mercury, he fixes at 0,05 the proportion of oxygen which is combined with the mercury; but for this determination, he supposes that on decomposing this salt by pot-ash, it is the pure oxide of mercury which is precipitated. But the metallic precipitates retain a portion of acid which he has neglected; I therefore believe, that according to his experiment, the proportion of oxygen ought to be rather greater than that which he establishes. I observe, that in many determinations made by chemists in modern times, this consideration has been neglected, which renders them in some measure uncertain.

The mild sulphate of mercury forms a permanent combination, which is not decomposed by water, as the oxygenated sulphate of mercury is, upon which I shall proceed to make some observations.

If the process indicated by Fourcroy be employed, that is to say, if the concentrated sulphuric acid be made to boil with the mercury, without letting it arrive at a state of desiccation, a white mass is obtained, composed of sweet sulphate and oxygenated sulphate. By the careful method of washing which he points out, the excess of acid that maintains the solution may be separated, from the oxygenated sulphate, and a portion of the mild sulphate.

When the operation is carried farther, or when a sufficient degree of heat is applied to the mild sulphate, a greater quantity of sulphureous acid is disengaged; the mercury becomes too much oxidized to form the mild sulphate, and the combination is found entirely in the state of oxygenated sulphate, which varies in the quantity of sulphuric acid it retains. Let us examine it in the state it possesses when the operation has been carried to dryness, in which consequently it may be considered as not possessing an excess of acid.

In this state water produces a separation; the mass which was before white, turns yellow; the liquid becomes very acid, and holds a part of the sulphate in solution; there is formed what has been called, since the time of Rouelle, a salt with excess of acid, and a salt with the least portion of acid; but the proportions of these two combinations vary, 1.

according

according to the quantity of acid that the first combination had retained; 2. according to the quantity of water employed; 3. according to the temperature; for the heat concurs with the action of the water.

If instead of simple water, an alkaline solution be employed, which exerts a more powerful action upon the acid than pure water, two different combinations are formed, one of which is found to be almost entirely the oxide, and the other in which the acid is for the most part engaged: the precipitate then afforded differs principally from that which water alone would have produced, by the smaller proportion of acid which it retains, and which depends on the degree of concentration of the alkaline liquor which acts upon it.

When the oxygenated sulphate of mercury has preserved a more considerable excess of acid, the action of the acid may be so weakened by the water, that no separation will be made.

3. I have hitherto supposed, that there were but two sulphates of mercury, one with the smallest possible proportion of oxygen, and the other with the greatest proportion; but it is manifest that there are but these two extremes that are fixed, so that they may contain within their limits all the other degrees of oxidation: the properties of these intermediate combinations differ so much as to leave no room for the determination of their particular properties, unless both the degree of oxidation, and the proportion of the acid be known.

What I here observe as to the intermediate degrees of oxidation, must also be applied to the other metallic salts, such as the sulphate of iron, in which there are likewise only two fixed terms, that of the weakest and that of the strongest oxidation.

4. Bergman had before discovered, that the solution of mercury by the nitric acid made in the cold, has different properties from that which is prepared by means of heat; and in his very excellent treatise on the analysis of waters, he remarks, that the former does not so readily afford a precipitate with the solutions which contain sulphuric acid, and that the precipitate it forms is white, whereas that of the solution made with heat is yellow; that the first then forms mild sulphate of mercury, which is white, and more soluble in water than the oxygenated sulphate, while the latter forms the oxygenated sulphate.

When the nitrate of mercury is prepared by means of heat, some nitrous gas is at first disengaged, but at a certain period, when the disengagement is complete, the mercury is perceived to be dissolved, with the production of scarcely any nitrous gas. I am indebted for this observation to Citizen Gay, a young chemist of the Polytechnic school, who adds much sagacity to considerable zeal. We see therefore, that by means of heat an oxygenated nitrate is formed, which if the operation be not stopped, afterwards combines with mercury, as in another operation the oxygenated muriate of mercury combines with a fresh quantity of this metal*. But in the fluid nitrate of mercury, there are no determinate proportions.

* By an observation of Fourcroy it is shewn, that the oxygenated sulphate of mercury submitted to ebullition with water and mercury, acts also upon the metal: it must therefore pass to the state of mild sulphate. The corrosive mercurial muriate, when dissolved in water, does not act upon mercury; but by trituration with mercury without water, it begins to combine with it, and to impart its oxygen: the combination becomes uniform by sublimation.

between,

between the mercury most oxygenated, and that least so : it appears that all the intermediate proportions may exist, as I have observed with respect to the sulphates.

When the nitrate of mercury is precipitated by the muriate of soda, mercurial muriates are obtained, which differ according to the oxidation of the mercury. With the nitrate little oxidized a white precipitate is obtained, which retains a part of the nitric acid, and cannot be dissolved in the muriatic acid, nor be taken up by the concentrated nitric acid; without giving out much nitrous gas. From the solution prepared by heat, a precipitate is obtained rather of a yellow colour, which is not soluble in the muriatic acid, but readily dissolves in the nitric acid, giving out little nitrous gas. The supernatant fluid affords by evaporation, a little corrosive mercurial muriate.

If the experiment be made with a nitrate, prepared in such a manner that the mercury shall be in the most oxidized state, and has not re-dissolved any metallic mercury, and if it be diluted with a considerably large quantity of water, no precipitate is formed, but all the mercury is found in the state of corrosive mercurial muriate: nevertheless, it is not obtained in this state but in a limited quantity, and sometimes even none is had, according to the proportion of muriate of soda employed, because the corrosive mercurial muriate has the property of forming with the nitrate of soda a quadruple salt.

This salt is capable of forming rhomboidal crystals, grooved on their face, of a considerable size: it fuses upon ardent charcoal; by exposing it to a sufficient heat in a retort, all the mercury is separated in the form of corrosive mercurial muriate. The residue is a nitrate of soda which retains a little muriatic acid, so that the separation which takes place is decided by the respective volatility of the substances, and by a difference of affinity between the nitric acid and the muriatic acids relative to the oxide of mercury.

After the crystallization of this salt, another is obtained in small needles, which appear to be a complex salt, in which the oxide of mercury is found in a greater proportion.

Nothing certain can be established as to the results of the mixture of the nitrate of mercury highly oxidized, and the muriate of soda, because they vary according to the proportions of the substances which act.

From the preceding observations we deduce, that the nitric solution of mercury may hold this metal in solution from the lowest degree of oxidation to the highest, or to that which is required for the constitution of corrosive mercurial muriate; that it may possess it in all the intermediate degrees, but that its properties will be different according to the degree of oxidation.

5. Fourcroy lays it down as a principle, that any metallic oxide whatever gives to acids a colour similar to that which it has itself; whence he concludes, that when a mercurial precipitate which proceeds from a white salt acquires another colour, a change must have been made in the oxidation. This opinion does not appear to me to be well founded.

I took some muriatic acid, and dissolved in it some red oxide of mercury; the solution was easily effected, without any disengagement either of oxygen gas, or oxygenated muriatic acid: it spontaneously afforded fine crystals of corrosive mercurial muriate. I may here
remark,

remark, that this process appears to me to be the most simple, and the least expensive for the preparation of the corrosive mercurial muriate.

The combination which I had formed contained the red oxide of mercury, with all its oxygen; yet it was colourless; with ammonia it would have given a white precipitate, and with lime and the alkalis a precipitate more or less orange coloured.

The red oxide of mercury readily dissolves in the nitric acid, without any disengagement of oxygen; this solution crystallizes and forms a white salt; but if there be not a sufficient excess of acid, it gives with water alone a white precipitate; with a greater quantity of water recently distilled, a yellow precipitate; with lime and the fixed alkalis a precipitate of a much deeper yellow colour.

A coloured oxide may therefore form white salts, and afterwards assume other colours, without undergoing any change in its oxidation.

6. Several chemists have observed, that the muriatic acid has a greater disposition to combine with very oxidized metals, than the nitric and sulphuric acids. Fourcroy applied this consideration with advantage to explain several phenomena. On this subject he thus expresses himself: (*Mem. de l'Acad.* 1790, page 381.) "Every acid requires quantities of oxygen in the metals, in order to combine with them; the muriatic acid in general does not combine with those metals, unless they be much loaded with this principle, or highly oxidized: the mercury appears to be more oxidized in the corrosive muriate than in the nitrate." I shall take the liberty to make some observations on the principles laid down by my learned colleague.

It does not appear to be exact to say, that each acid requires different quantities of oxygen in the metals to combine with them; the nitric, the sulphuric, and the muriatic acids form combinations with mercury, from the least to the most oxidized terms; and it is the series which results in the muriates, which makes the principal difference in all pharmaceutical preparations, from the corrosive sublimate to the mercurial panacea: but there is this difference, that the combination of the sulphuric acid and the nitric acid with highly oxidized mercury is much weaker, and more easily decomposed, even by the action of water, than that of the muriatic acid, which on the contrary presents a very permanent constitution.

If our attention be directed to those metals which have the property of assuming large proportions of oxygen, such as iron, tin, antimony, &c. we shall observe the same properties with regard to the sulphuric, nitric, and muriatic acids, so that the action of the two first, which diminishes in proportion as the oxidation advances, is sometimes so much weakened, that they abandon entirely, or are incapable of dissolving some very oxidized metals; on the contrary, the muriatic acid dissolves them and holds them in solution, so that it cannot be perceived whether its action be weakened, or whether on the contrary, it be not increased by a greater oxidation.

A plausible explanation may be given of this comparative property of the sulphuric, nitric, and muriatic acids. Sulphur and azote, the bases of the two former are saturated with
3. oxygen;

oxygen, so that their resulting affinity for the substances that are also greatly oxygenated is very weak; but the muriatic acid which appears to have in its constitution only a very small proportion of oxygen, ought to have a much greater disposition to combine with oxygenated substances.

7. In the preceding observations I have supposed, that the different alkalis share the acid of a metallic solution with the oxide which is precipitated. I do not here speak of precipitates by ammonia, which in some circumstances is decomposed, and by that means changes the constitution of the precipitate, as has been particularly shewn by Fourcroy; but this property is subject to modifications which demand particular attention.

The experiments of Bayen have ascertained beyond a doubt, that the precipitates of mercurial nitrates and muriates retain a greater or less portion of acid, so that on exposing the precipitates of the mercurial nitrate to a sufficient heat, some nitrous acid is disengaged; and on making the same experiment on the precipitates of mercurial muriate, a greater or less quantity of insoluble mercurial muriate is sublimed. I shall here add some observations to those I have already made. (*Inquiries*, Art. XIII. No. 1.)

When the oxygenated muriate of mercury is decomposed by the precise quantity of carbonate of soda, which is necessary to effect its precipitation, the precipitate contains muriatic acid, carbonic acid, and oxide of mercury in excess; so that on exposing the precipitate to the action of heat, carbonic acid gas and oxygen gas are disengaged; nearly half the mercury is reduced to metal, and the rest of the precipitate is sublimed, retaining all the muriatic acid which remained in combination with the oxide, and forming the mercurial muriate observed by Bayen. The liquid supernatant over the precipitate affords by evaporation, at first carbonate of soda, and afterwards a triple salt in which soda predominated.

The carbonate of pot-ash presents different phenomena. A very small portion only is necessary to produce a total precipitation in the solution of corrosive mercurial muriate, and the precipitate obtained does not amount to half the weight of what would be given by the same quantity of oxygenated muriate of mercury, precipitated by the carbonate of soda. This precipitate, exposed to the action of heat, gives out carbonic acid, and sublimes almost totally in the state of mercurial muriate, with the least portion of acid; a very small portion only resumes the metallic state. The liquid that covers the precipitate makes no effervescence with the acids; so that here the whole of the carbonic acid is combined in the precipitate with the oxide, and a portion of muriatic acid nearly double that contained in the precipitate by carbonate of soda. The liquid when evaporated affords a triple salt much more soluble in water than the oxygenated muriate of mercury: this salt crystallizes in silky needles.

The carbonate of ammonia causes an effervescence on decomposing the corrosive mercurial muriate, and no acid is found either in the precipitate or in the supernatant liquid. The weight of the precipitate constitutes nearly five sixths of the muriate decomposed; a disengagement of ammonia takes place on the addition of lime. When urged by heat, the ammonia is decomposed, and azote gas only is received. All the mercury is sublimed with-

out

out reduction. But we perceive that in this sublimate the mercury cannot be as much oxidized as in the sublimation of the precipitates by the fixed alkalis or lime, because it has lost a part of its oxygen by the decomposition of the ammonia. The liquid which covered the capitate contained a combination of oxide of mercury, of muriatic acid, and ammonia.

The precipitation by ammonia presented the same phenomena, except the effervescence.

8. By directing our attention to the other metallic solutions and precipitations, the character of those of mercury are easily discovered, as well as the modifications which depend on the particular affections of each oxide; I therefore think we are justified in stating the following principles:

1. The acids act upon the metallic oxides the same as upon the other substances, in proportion to their mass, since when a metal has become but little soluble or insoluble, it may be dissolved by an excess of acid, or may form, by means of such excess, a more durable combination.

2. When a metallic combination is decomposed, the alkali or alkaline earth made use of, produces a division of the acid, in proportion to the energy of its action. If the metallic combination is weak, water is sufficient to decompose it; salts with either the maximum or minimum of acid are then formed. In this case the metallic oxides follow the same laws as other substances; but it sometimes happens that it is not the alkaline base which is added that assumes its share of the acid, but it is the metallic oxide on the contrary that divides the precipitate with the acid; as when we form fulminating gold, or the orate of ammonia. Sometimes also the precipitant, the acid, and the metallic oxide form two complex combinations; one of which is insoluble and the other remains liquid, as we have seen in the decomposition of the corrosive mercurial muriate by ammonia, and in the experiments that I have described (first series, Nos. X. and XI.)

In general, but particularly with respect to the metallic solutions, we must not separate in our reasonings, the substance employed by the name of precipitant from the liquid in which the precipitation is effected; but attention ought to be paid equally to all the substances presented, and which may form new combinations.

3. The coloured oxides may produce colourless combinations; but on giving out a part of the acid with which they were combined, their colour will again appear in proportion to the quantity of acid they have yielded; so that this colour is an indication of the constitution thus established, provided the state of oxidation has not been changed by any particular circumstance.

4. The metallic oxides cannot be compared together, unless they are taken in a determinate state of oxidation. All the combinations they are capable of forming vary, not only from this cause, but also from the proportion of acid they retain, when this proportion is not determined by a crystallization. With respect to oxidation there are only two extremes; that of the smallest, and that of the greatest oxygenation which can be considered as constant.

Hence it follows, that the nomenclature can only indicate in a vague manner, and with great latitude, any metallic combinations in which the oxidation and the proportion of the acid are not determined.

5. The acids do not follow the same progression in their affinity relative to the degrees of oxidation. There are some in which the affinity diminishes with the oxidation; such are the nitric and sulphuric acids; in others again it appears to increase, such as the muriatic acid. Hence we may perceive, independant of all the considerations I have mentioned, how groundless the pretension was of classing the affinities of metals for the different acids, by considering them as constant forces.

6. The results of the complex affinities of metallic solutions, mixed with other salts, may also vary from the proportion of those salts, conformably to what has been observed (*on the influence of proportion in the complex affinities*); so that then the proportion of oxygen in the metallic oxide, that of the oxide in the solution, and that of the saline combination brought into action with it, all contribute to the new combinations which are established.

7. We might therefore observe in the chemical action of the metallic solutions, the laws which we have established in that of the other combinations, if oxidation did not cause a change in the affinity of the metal, either for the acids, or for the other substances, and multiply, as it were, in the metals the property of forming combinations; whence the results are often so complicated, that though by accurate observation we may develop the facts and the circumstances which determine them, yet they cannot be foreseen by theory, from the mere consideration of the known properties, as may be done with respect to other substances, the action of which depends on a smaller number of conditions.

8. The state in which oxygen is found combined with the metals, has likewise an influence upon the properties of the metallic oxides, and of the precipitates compared with each other. Bayen observed, that several mercurial precipitates, as well as the red oxide, detonate strongly, though in an unequal manner when exposed to heat, after having been mixed with sulphur, and that some do not possess this property; but he has not given the reason of the phenomenon and the exceptions. It appears to me indubitable, that this property of the oxide of mercury, and of the precipitates in which it predominates, is a consequence of the oxygen possessing more caloric, than it preserves in the combination that it forms with the sulphur, or in the sulphuric acid. The same thing therefore happens as with the nitrate and oxygenated muriate of pot-ash; except that the effect is less considerable; but in the precipitates of the corrosive mercurial muriate, part only of the mercury can be considered as not combined with the muriatic acid, namely, that part which may be reduced into metal by the action of the heat which produces detonation. Such of the precipitates therefore as retain a sufficient quantity of the muriatic acid, to admit only of a small reduction of mercury by the action of heat, cannot produce detonation; and such in fact are the precipitates by ammonia, by the carbonate of ammonia, and by the carbonate of pot-ash.

9. The

9. The property discovered by Bayen is therefore analogous to the fulminating property of the orate and the argentate of ammonia; but it is not perceptible in the other metallic oxides and precipitates. Hence the oxygen in the latter must necessarily contain a less portion of caloric.

10. The properties which depend upon oxidation vary therefore in each metal, on account of the proportions of oxygen; and those which depend on the state of concentration possessed by oxygen, constitute the leading cause of those phenomena which arise from the changes of combination in elastic substances. (*Inquiries*, Art. XIV. Nos. 12 and 15.)

(*To be continued.*)

VIII.

Experiments and Observations on various Kinds of Candles, made of Wax, Tallow, Spermaceti, and of such as are manufactured of a Mixture of these Substances; as well with Regard to Cheapness as to the Quantity of Light they afford. By Sig. FR. HERMSTAEDT, Professor and Member of the Royal College of Physicians at Berlin.*

THE illumination of apartments is an article of the first necessity for every class of people, and the means by which it is produced are so likely to advance in their price, that this illumination continues to be an object of considerable importance, for every family in housekeeping, whether numerous or moderate. But as it is impossible to obviate this necessity for lighting our apartments, it becomes the duty of every one to be attentive to the prospects of oeconomy in this respect: since the saving, though small with regard to a single candle, may nevertheless in time amount to a sum by no means contemptible in housekeeping upon a larger scale; more especially, when the elegant enjoyments of life demand a brighter light, than is necessary for the mere purposes of domestic utility.

The experiments which I lately had an opportunity of making during the official examination of various sorts of tallow candles, with regard to oeconomy in their consumption, as well as to the quantity of light afforded by their combustion, afforded results, which appeared deserving of attention, and were sufficient to induce me to make a new set of experiments, with candles made of mixed ingredients; in order to compare them with the former. These, likewise, furnished results by no means uninteresting, and afford conclusions of equal utility. These reasons have induced me to publish my observations for further examination; and even should my remarks and observations appear to some readers of very subordinate importance, yet I might request them to consider, that even facts of little moment have frequently afforded a foundation to more discoveries of greater magnitude.

* Translated from the German in *Scherer's Allgemeinen Journal der Chemie*, Vol. IV. page 40.

First experiment with a wax candle of the size of six to a pound.—A white wax candle weighing exactly 2 ounces 11 drachms ($5\frac{1}{4}$ Loth.) *avoirdupois weight**, was lighted and kept burning for one hour in a calm place; after which combustion it exhibited a loss of weight of 115 grains. According to this proportion the combustion of the whole candle would have required 11.217 hours, or which is the same, 11 hours and 13 minutes. The burning of this candle proceeded quietly, with a tranquil, not undulating flame, and without notable smoke and foot.

Second experiment with a mould tallow candle of six to the pound.—The candle weighed 2 ounces, 10 drachms, ($5\frac{1}{4}$ Loth.) and lost in weight 185 grains after an hour's burning; the whole of it would therefore have been intirely consumed in 6 hours and 45 minutes. The flame was slightly agitated, and emitted much vapour.

Third experiment with a mould tallow candle of eight to the pound.—The weight of this candle was 1 ounce, 15 drachms, and 4 grains ($3\frac{7}{8}$ Loth.) When it had burned one hour, it had lost 140 grains; whence it might have burned 6 hours and 40 minutes. The other phenomena during its combustion were like those of the preceding.

Fourth experiment with a mould candle of nine to a pound.—Its weight was 1 ounce 12 drachms, ($3\frac{1}{2}$ Loth.) and the loss of weight after one hour's burning was 140 grains; consequently this candle would have lasted seven hours before its intire consumption. The flame resembled that of the last.

The results afforded by these experiments appear to me deserving of attention in various points of view. It was on this account that I resolved to make various kinds of candles, composed of pure spermaceti, of spermaceti and tallow, and of tallow and wax, in various proportions of the ingredients. And lastly, I made trial of a mixture of wax, tallow, and spermaceti. The results which I obtained in these experiments were the following:

Fifth experiment with a mould candle, made of two parts of tallow and one of wax.—This candle weighed $2\frac{1}{2}$ ounces, 24 grains (5 Loth.) It was lighted, and after an hour's combustion lost 166 grains; it might consequently have burned for the space of 7 hours and 22 minutes. The flame of this candle was a very good one; it emitted scarcely any vapour, and in this respect it very much resembled the candle made of pure wax.

Sixth experiment with a candle manufactured of one part wax and three parts of tallow.—The weight of this candle was 2 ounces and a half, and 34 grains. From the loss of 193 grains, which it sustained by burning one hour, it appears that it would have required 6 hours and 23 minutes to be wholly consumed. The flame of this candle was likewise steady, and afforded but a small quantity of foot.

Seventh experiment with a mould candle made of pure spermaceti.—I made a candle of this description, of the weight of 2 ounces and 3 drachms, ($4\frac{1}{2}$ Loth.) When lighted and kept

* The *Berlin pound* of 32 Loths, or 16 ounces, is equal to 3616.3 English grains; so that one ounce Berlin is equal to 226, and the drachm to $14\frac{1}{4}$ English grains, disregarding the small decimal fraction of 3-10ths of a grain in the pound. The subdivision of the English *avoirdupois weight* into 16 ounces, each into 16 drachms has been employed here without farther reduction.---Transl.

burning for one hour, it lost 250 grains of weight, so that the whole time of its combustion would have amounted to 4 hours, 12 minutes. Whence it follows, that the candles made of spermaceti are far less economical with regard to expence, than wax or tallow. Besides which, the flame produced by this candle was very unsteady, fleeting, and it also gave out a quantity of smoke and foot.

Eighth experiment with a candle composed of equal parts of spermaceti and tallow.—This candle weighed 2 ounces, 6 drachms, and 4 grains, ($4\frac{3}{4}$ Loth.) I suffered it to burn one hour, and found it to have lost 213 grains; whence the whole time of its combustion would have been 5 hours 22 minutes. Its flame differed very little from that observed in the foregoing experiment.

Ninth experiment with a candle made of one part spermaceti and two parts of tallow.—The weight of a candle of this mixture was 2 ounces, 6 drachms, and 5 grains, ($4\frac{3}{4}$ Loth.) of which it lost 200 grains by one hour's burning. Its total combustion, therefore, would have been accomplished in 5 hours, and $43\frac{1}{2}$ minutes. This candle guttered or overflowed very much, and afforded a bad, footy flame.

Tenth experiment with a candle prepared of one part of spermaceti mixed with three parts of tallow.—This candle weighed exactly 2 ounces, 6 drachms, and 45 grains, ($4\frac{3}{4}$ Loth.) By burning for one hour, it sustained a loss of weight of 235 grains; hence the whole time of its combustion would have amounted to 5 hours and 3 minutes. The flame of this candle also was not notably different from that of the last.

Eleventh experiment with a candle composed of equal parts of wax and spermaceti, together with three parts of tallow.—The weight of this candle amounted to 1 ounce, 6 drachms, and 56 grains, ($3\frac{3}{4}$ Loth.) During the combustion for one hour it lost 246 grains, and would consequently have lasted 3 hours, 54 minutes, before it was intirely consumed. In other respects the flame of this candle was very bad; and it deposited a quantity of smoke and foot.

Computation of the Expence attending the Consumption of these different Kinds of Candles compared with each other.

In order to obtain a very accurate proportion of the saving of expence that might be made by burning these various species of candles, it would be necessary, not only that the candles themselves should consist of equal masses, but likewise of equal diameters or circumferences: for since, as I shall prove in the sequel, it results from my experiments, that the saving in the burning of a candle is proportional to the smallness of its diameter, and since the candles employed in experiment 3 and 4 had a smaller diameter than the others, the following calculation with regard to the œconomy in burning these two species, is only an approximation; whereas it is perfectly accurate with regard to all the others, because they were used of exactly the same diameter. It will be easily perceived, that I have calculated the saving to be made in the burning of a candle from the quantity of fuel

or combustible matter, which is consumed in a given time. Besides which, I have assumed for the given time the space of an hour; and in order to provide myself with a determined point of comparison, I have taken that quantity of fuel which is consumed in one hour by burning a wax candle, for the given unity as a base, and have thus compared the respective losses in the quantities of combustible matter, which the other sorts of candles sustain, while burning during an equal space of time. The results of this calculation are, for the sake of brevity, placed together in the following table:

If, in a given time, are consumed of a pure wax candle - - - 1000 Pounds:

There will be consumed during an equal time of

a . . . tallow candle, 6 to a pound	- - - - -	1,608
b . . . ditto 8 ditto	- - - - -	1,217
c . . . ditto moulded, 9, ditto	- - - - -	1,043
d . . . candle made according to the 5th experiment	- - - - -	1,443
e . . . ditto - - - - - 6th ditto	- - - - -	1,678
f . . . ditto - - - - - 7th ditto	- - - - -	2,174
g . . . ditto - - - - - 8th ditto	- - - - -	1,852
h . . . ditto - - - - - 9th ditto	- - - - -	1,739
i . . . ditto - - - - - 10th ditto	- - - - -	2,008
k . . . ditto - - - - - 11th ditto	- - - - -	2,138

From these results it is very manifest, that of all candles those made of pure and white wax burn the longest, and also that tallow candles are the more sparingly consumed, the thinner they are, or the smaller their diameter is, and that, on the contrary, spermaceti candles undergo the greatest waste of fuel; besides, that they emit more vapours than candles of tallow, although those vapours have not a very disagreeable smell. He therefore, who desires to illuminate his room with economy, should procure very thin, mould tallow candles, so that about 12 or 16 of them will go to a pound, and he will find them still more advantageous, than is shown by the above result; since, in it, mould candles of only 9 to the pound have been reckoned. But even in that case we suppose, that in a certain rate of house-keeping 100 pounds of tallow candles of the size of 6 to a pound are required, the same illumination may be effected, according to the experiment here alluded to, with only $64\frac{3}{4}$ lb. if dipped candles of 9 to a pound be employed. Now the pound of mould candles costs 5 *grofschen* *; consequently 100 pound will cost 20 dollars, 20 *grofschen*, whereas $64\frac{3}{4}$ lb. of dipped candles, reckoning the pound at $4\frac{1}{2}$ *grofschen*, will cost no more than 12 dollars 3 *grofschen* $4\frac{1}{2}$ d. so that in such a housekeeping there is a yearly saving of 8 dollars, 16 *grofschen* $7\frac{1}{2}$ d. which in another upon a larger scale will come to a considerable amount. It need not be mentioned, being obvious of itself, that in thinner candles the thickness of the wick must always bear due proportion to the mass of tallow.

* The Berlin *grofsche* is equal to 14-15ths of a penny, or nearly one penny and a farthing English. The *grofschen* is subdivided into 12 pennies, and one Berlin dollar is worth 2 shillings 6 2-5ths. pence at par.

We may now from the preceding results, founded on experience, draw the following conclusions :

a) That in general tallow candles of a smaller diameter are consumed more sparingly than thicker ones.

b) That the reason of this phenomenon must be sought for in the mass of tallow, which being too great in thick candles, is heated at once, and thus volatilized without burning like the fat in roasting meat; for which reason,

c) Since a sufficient quantity of oxygen gas from the atmosphere cannot then act upon it, but only on the exterior part of the flame, a great part of the combustible matter must be lost in these vapours, at the increasing intensity of heat, without decomposing the air in order to extricate light by burning.—But this on the other hand,

d) Cannot be the case with thinner candles, because much less of fuel is lost, and also because in that case less of tallow is necessary to be consumed, in order to produce the same quantity of light as is afforded by a thicker candle: for in this case more of oxygen gas is decomposed, and more free light separated, which also

e) Affords this great advantage, that such a candle produces less vapour and foot, because the fuel being then more effectually consumed, will be volatilized, and go off as foot in an undecomposed state in a much less proportion.

But candles would afford the brightest and purest light, which have a broad, ribbon-like wick instead of a round one, or to which the form of hollow cylinders were given, so that the air might act upon the flame inwardly as well as outwardly. For by an arrangement of this kind all the combustible matter will be consumed, and no smoke nor foot, but merely water and carbonic acid gas can be formed, as must be evident to every one acquainted with the principles of chemistry. But to such as are not chemists, Argand's lamp may serve as a proof. At the same time it is equally evident from the preceding theory or explanation, that every flame, which is not obscured by foot, must afford a stronger light than another in opposite circumstances; and it is equally obvious, that such a flame, from the total absence of smoke and foot, cannot emit any unpleasant odour. I intend to institute further experiments on this subject, and to communicate them at a future time.

Lastly, I have also endeavoured to ascertain the intensity of light emitted by the various sorts of candles here mentioned. For this purpose the lighted candle was placed upon a table in a room totally darkened. I then, holding a book in the hand, withdrew backwards, until the characters became invisible to my eyes. The several distances from the luminous cone were accurately measured, and afforded the following results:

The wax candle (Experiment I.) emitted light, of which light the illuminating distance was 10 feet, 1 inch, 6 lines Rhinland measure*.

* A Rhinland foot is equal to about 1 foot and 3-10ths of an inch English measure.---A line is one-twelfth of an inch.---Transl.

					Feet	Inch.	Lines.
The tallow candle of Experiment II.	gave	-	-	-	11	2	6
The ditto ditto III.	-	-	-	-	11	5	0
The drawn candle of Experiment IV.	-	-	-	-	12	2	3
The candle of ditto V.	-	-	-	-	12	9	0
The ditto ditto VI.	-	-	-	-	11	3	0
The ditto ditto VII.	-	-	-	-	9	9	0
The ditto ditto VIII.	-	-	-	-	12	6	0
The ditto ditto IX.	-	-	-	-	12	4	6
The ditto ditto X.	-	-	-	-	12	6	0
The ditto ditto XI.	-	-	-	-	12	9	0

If these distances be compared with each other, and if the intensity of illumination shewn by the wax candle be taken as the standard unity, the following intensities of light, as produced by the several candles, will be the result by calculation.

If the illumination, effected by the burning of the wax candle according to Experiment I, be

								1,000
then that of Experiment II. will be	-	-	-	-	-	-	-	1,107
ditto III.	-	-	-	-	-	-	-	1,127
ditto IV.	-	-	-	-	-	-	-	1,203
ditto V.	-	-	-	-	-	-	-	1,260
ditto VI.	-	-	-	-	-	-	-	1,104
ditto VII.	-	-	-	-	-	-	-	0,963
ditto VIII.	-	-	-	-	-	-	-	1,234
ditto IX.	-	-	-	-	-	-	-	1,234
ditto X.	-	-	-	-	-	-	-	1,234
ditto XI.	-	-	-	-	-	-	-	1,206

From this it follows, that, in general, tallow candles burn with a brighter flame than those made of wax; notwithstanding that the results of these experiments do not seem to succeed each other in a perfectly regular order. On this account I do not yet venture to infer from these results any decided opinion concerning the strength of light; but shall postpone the fuller investigation of this point to a future time and more leisure*.

* It is hoped the author will then likewise take notice of the thickness of the wicks, as well as of the temperatures, size, and other circumstances of the rooms where his experiments were made, which on one side will make his inquiry more complete, while on the other it will afford a means to form by calculation a reasonable estimate of the probable quantity of such kind of fuel wasted by combustion in a given time, place, and temperature. There is also reason to conclude, that a greater degree of accuracy would have been obtained if the relative illuminations had been determined from the shadows of the same book when equally dark upon an opposite wall, as the intensities of light are then universally as the distances of the candles from the wall.---Translator.

IX.

Short Notice concerning the Properties and external Characters of some new Fossils from Sweden and Norway; together with some Chemical Remarks upon the same. By Mr. D'ANDRADA. In a Letter to Mr. Beyer, Master of the Mines at Schneeberg.

SIR,

WHEN you had the goodness to inspect some specimens of the new fossils, which I have found in my last travels in Sweden and Norway, and brought along with me, you desired to become acquainted at least with their number and names. I comply with this suggestion; but wish, at the same time, I were also able to communicate to you a description in my own manner, as well as the results of the analyses which I have already made of some of them, together with that of others, which at present are the object of my occupation, and of those of which Professor Abilgaard has undertaken the analysis at Copenhagen. However, having destined those descriptions and accounts partly for the Academy of Sciences at Stockholm, partly for Copenhagen, partly for the Berlin Society of Friends inquiring into Nature, and partly for the Philomathic Society at Paris, I must for the present deny myself the pleasure of describing them fully.

You will receive, together with this letter, only a few of these species of fossils, together with a short notice concerning their properties and characters.

I. ACANTHICONE.

The colour is leek and olive green, and sometimes, though seldom, that of the greenfinch. Specific gravity from 3.4075 to 3.3562; but that of the more compact sort 3.3000. It is not scratched by quartz, and it gives fire with steel. The texture of the mixed specimens, or in lumps or crystallized, is foliated, but in the others fine splintery, and more compact. The laminæ are thin, very coherent, and of a three-fold crossing.* The entire splinters are quadrangular parallelopipedons with oblique terminations. On the edges it is transparent. The internal lustre is of the vitreous kind. The crystals are, (1.) quadrilateral, hexahedral and decahedral columns, terminated in dihedral, tetrahedral, and hexahedral pyramids; and sometimes also without pyramidal terminations. (2.) Tables or plates, quadrilateral, oblong, rhomboidal, sharpened off on the long narrow lateral facets.

* *Durchgang* in the German. Many fossils are capable of being split in various directions; so that the laminæ cross or tranverse each other under various angles, and hence each particle of such a fossil must belong to one or more laminæ. If these cut each other in one direction only, as in mica, talc, &c, it is called *single crossing*; if in two directions, as in the feld-spar, hornblende, hyacinth, it is called *twofold crossing*; if in three, as in spathose iron ore, ponderous spar, galena, &c. it is then called *threefold crossing*, and so on. See *Emmerling Lehrbuch der Mineralogie*, 1793, Vol. III. 479.---Transl.

If two of these plates be rubbed upon each other, they are a little phosphorescent, and emit a smell resembling that of quartz by friction. Acanthicone is in some degree pyro-electrical. Before the blow-pipe upon charcoal it readily fuses, with effervescence, into a blackish scoria replete with air-bubbles. In the cold it is insoluble in the sulphuric and nitric acids. This fossil occurs in the Swedish iron-mines, near Persberg, Lengbanshytta, and Norberg; but in Norway very beautiful specimens of it are met with in the Kulstad-mines at Helgoland, and near Arendal in the iron-mines Tornbjørnsbo, Ulrica, Notbro, &c. Till now it was mistaken, or confounded with schorl, or with green garnets. Some crystals are so large as to weigh five pounds.

II. SPODUMENE.

The colour is commonly a greenish-white of various shades. The lustre that of the mother-of-pearl. In the substance itself it is little transparent, but much so on the edges. Its specific gravity is 3.218. It scratches glass, but is itself scratched by quartz, and affords a white powder. It feels colder than quartz, yet dry and smooth. Spodumene is not at all electric, nor phosphorescent; nor does it afford a quartz smell by friction. Its texture is lamellar, with a double crossing. Its perfect fragments are rhomboidal acute-angled prisms (120° and 55°). The fragments of the cross-fracture are for the most part longish plates, that have their edges but little sharpened. When subjected to the blow-pipe upon charcoal, it becomes, at the first gentle impression of heat, opaque, dull, and yellowish; then develops itself in the direction of its laminæ, at the same time that it swells a little, falling afterwards into an insipid powder, which by a stronger heat affords a very transparent greenish-white glass. Nitric acid does not dissolve it, nor produce any effervescence. It is found, together with several other fossils, in the remarkable formation of iron at Uton, in Skarrgarde, three miles from Dalero.

III. SAHLITE.

Its principal colour is a pale asparagus green of various shades. Its external lustre vitreous, inclining to wax, but internally it has little splendor. It is transparent, and sometimes semi-pellucid, if the crystals are pure, and have suffered no decay. Specific gravity 3.2368. Sahlite barely marks glass, and does not strike fire with the steel, and is easily broken. Its texture presents straight and smooth lamellæ, thrice crossing each other in a somewhat acute-angled direction. The fragments of the cross-fracture are roundish. Their form of aggregation is of the coarse-grained kind, and sometimes, if the stone be in lumps or masses, they are hamated, or indented into each other. This stone occurs in crystals of rectangular, quadrilateral columns, the lateral edges of which are slightly truncated, and convex. The broad terminating edges of these columns are slightly truncated. The streak by rasure is white. This stone is somewhat soft and idio-electric. When rubbed upon a piece of the same kind, it emits no odour, nor shews any phosphorescence.

effluence. Before the blow-pipe it is infusible. It is found in Sweden, in the Sala silvermine in Westermannland. I have likewise discovered it in Buoen, three quarters of a mile distant from Anen, in Norway, where it occurs in masses, straight and thick foliated.

IV. ICHTYOPHTALME.

The chief colour yellowish-white. Lustre like mother-of-pearl, approaching to greasy. This fossil is transparent. Its specific gravity is 2.491. It scratches glass and easily admits of being filed; affording a white powder of a rough feel. Its own substance feels smooth, but not very dry, and is as cold as quartz. It is very difficult to pulverize it. Texture lamellar, of a more than triple crossing. When crystallized the laminae are large; but they are small when the fossil is massive, or exhibits a splintery fracture. The form of the crystals cannot be determined, as they are very much concreted, and strongly cohere with each other. The fragments of the transverse fracture are irregularly angular, in some instances orbicular with sharp edges. When first acted on by heat upon the charcoal before the blow-pipe, it undergoes no change, not even with respect to its colour; but in the more violent second heat the pointed edges run into a white enamel. With sulphate of lime (gypsum), the ichtyophthalme is infusible; on the contrary, it runs, when mixed with fluat of lime (fluor-spar), into a semi-pellucid milk-white glass, the fusion being accompanied by a little effervescence. This fossil occurs at Uton, in Sweden, and consists of siliceous and a little alumine, or argillaceous earth.

V. COCCOLITE.

As to colour, coccolite is mountain, grass, and olive-green. Its lustre is vitreous and resplendent. It is opaque, and its specific gravity is 3.316. It scratches glass, but excites only a few sparks with the steel. Its streak is grey, or greyish-white; its texture broad foliated, in which a single crossing of the laminae is observable. It is an aggregate of large coarse and fine grained, granular polyhedrons. These grains appear in some instances to be short quadrilateral columns, sharpened at both ends, and having pyramidal terminations joined to, or superimposed upon, the lateral surfaces. This stone is infusible by itself. With carbonate of pot-ash it fuses with effervescence, and swells to a frothy scoriaceous glass, of a dirty olive-green colour; and with borax it produces a pale-yellowish semi-pellucid glass. It is met with in the iron-mines Hellesta and Asebro, in Sudermannland, as well as in Neriké, in Sweden, and likewise in a beautiful form in the Arendal iron districts of Norway.

VI. APHRIZITE.

Colour black, somewhat greyish. External lustre vitreous, inclining to the gloss of fat; internally a little resplendent. Aphrizite is opaque. Its specific gravity is 3.1481. It gives fire with steel, and cannot be filed; is very brittle, and readily broken. Its texture is compact. The cross fracture is smooth, a little even, inclining to the flat conchoidal;

and the fragments of it amorphous, yet angular and of sharp edges. The crystals are hexahedral, short and thick columns, which sometimes, from the different truncatures of the edges, have the appearance of dodecagonal columns. Both species are terminated in tetrahedral pyramids. This stone is feebly idio-electric, but not at all pyro-electric. It intumesces before the blow-pipe on the very first action of heat, frothing at the same time, and yielding a greyish or yellowish white glass. With borax it effervesces, foaming strongly, and produces a greenish white pellucid glass. It occurs at Langoe, a small island not far from Krageroe, in Norway.

VII. ALLOCHROITE.

Allochroite possesses a yellow-grey, and, in some instances, a dark straw-yellow colour. It has little lustre of the vitreous kind, which in the recent fracture passes into that of wax. It is opaque. Its specific gravity is 3.5754. It is just scratched by quartz, gives fire with steel, and is not easily broken by the blow of the hammer. Its texture is compact, and it is met with in large, thick slaty plates, with a decayed yellowish-white surface. Fracture uneven, of the small and perfect conchoidal kind. Fragments angular and indeterminate, not much sharpened at the edges. It is infusible alone as well as with borax of soda (borax). When treated with microcosmic salt (phosphate of soda and ammoniac), it exhibits an enamel-like, more or less perfectly fused surface, which, on gradual cooling, shews at first a reddish-yellow, then a green of differently deep tints, and at last a dirty yellowish-white colour. This change of colours seems to indicate some metallic ingredients. The native place of this stone is the mine Wirum, in the vicinity of Drammen, in Norway.

VIII. INDICOLITE.

The colour of this stone is a dark indigo-blue, a little lighter in the fracture, so as to incline to the azure or sky-blue. Its external lustre is vitreous in a high degree, approaching to the metallic splendor. It is untransparent, and not very heavy. Its specific gravity cannot be accurately ascertained, on account of the small crystals bedded in it. Quartz is a little scratched by it. It is easily broken. The streak is blueish-grey. It feels cold and dry like feld-spar. Its texture appears to be compact; but the longitudinal fracture is finely striated, and the cross fracture somewhat uneven, passing into the small conchoidal. Its crystals are rhomboidal columns, much striated lengthways. The fundamental form of crystallization seems to be quadrilateral; but for the most part these crystals are polyhedral, needle-shaped and stellular. It does not fuse before the blow-pipe. It is found near Uton, in Sweden.

Note. This fossil resembles in its colour the lazulite of Professor Klaproth, which I know only by description: but as to its other physical and chemical characters it differs from it.

(To be concluded in our next.)

X.—*Experiments*

X.

Experiments on the Conducting Power of Fluids with Regard to Heat.—W. N.

AMONG the papers presented to the philosophical world since the commencement of the present Journal, none occupy a higher rank than the experiments and deductions of Count Rumford on heat; whether we attend to their extensive and important consequences, or the powers of philosophical investigation they exhibit. One of the most singular facts he has noticed is the slowness with which heat is conducted through fluids; a fact long ago pointed out and strikingly impressed by him, and latterly rendered more remarkable by his inference, that bodies in the state of fluidity have absolutely no power of conducting heat from particle to particle, but convey it from one solid to another merely by their motions or currents. Whether this inference taken in the extreme of strictness could be justified by his own, or indeed by any experiment, was doubted by myself as well as others; but without treating the subject in that precise manner, it becomes a question of some interest and curiosity, to decide whether the direct conducting power of fluids be too minute to be ascertained by careful experiments more or less varied from those of the Count. Doctor Thomson, of Edinburgh, is, I believe, the first who undertook and conducted a process of this kind, an account of which is given in his able paper in our last volume, p. 529. I need not here advert to the particulars of that memoir, farther than to remark, that in the discussion of this subject in conversation by some philosophical friends who meet weekly at my house, our consideration was more particularly directed to the argument which is drawn at p. 534, in favour of the condensing power of fluids from the Count's own experiments. It is there observed, that when ice was slowly melted by boiling hot water standing over it, with the interposition of a thin stratum of ice-cold water, there were undoubtedly currents produced through this last stratum, because water becomes denser either by receiving or losing heat till it arrives at 40° ; but it is contended, that the heat which passed to the cold water in this process must have been conducted from particle to particle, and not conveyed by currents of the superior water; and farther, that the regular diminution of the numbers expressing the temperatures of the strata of water from the surface downwards, is inconsistent with the supposition of such currents, which it should seem ought to produce a mixture of the fluid.

It appeared to us, that the principal question in this case was, whether or not the whole conveyance of the heat were effected by the mediation of the vessel or solid; which it ought to be if the solid be the only proper and perfect conductor present? That is to say, if heat be applied to the surface of a fluid, and the effect of that heat be to produce rarefaction, it must inevitably follow, in this simple statement of the case, that no heat would pass below the range of particles at the very surface where the heat was applied, unless the fluid itself were properly and strictly a conductor.

But

But as the case is not so simple, but must necessarily include the vessel, or solid coating of the fluid mass, the consequences will be different. If the fluid be capable of receiving and giving heat only from and to solids, we must contemplate the heated parts as rarefied, (that is to say increased in bulk) and consequently diffusing themselves laterally over the rest of the surface; as coming into contact with and heating the vessel; as urged to descend at the circumference by the successive arrival of other more heated particles, and consequently converging again beneath, and ascending near the center; we must consider the solid side as conducting the heat still farther down; and there causing an ascending current to join its water to the converging streams above. So that the water may be considered as receiving heat from a solid in contact with its surface near the center; giving it again to a solid at its circumference; and this last solid in its turn heating another stratum of water beneath. As the vessel should become heated to a greater distance downwards, the effect would be more perceptible to a greater depth in the fluid; and from the want of communication between the particles among each other, the ascents and convergences might have different velocities, according to the temperatures at the several depths, and preserve different temperatures in the several strata regularly increasing according to the advancement of the operation.

We thought it probable, as the passage of heat downwards is very slow, that these currents, if they exist, might also be so slow that different temperatures might be found at one and the same time in the same stratum; and we supposed the experiment would be more conclusive, if the heating solid were made to cover the whole surface of the fluid, and to rest upon the vessel itself. For in this arrangement, as the upper edge of the vessel would be as hot as the surface of the fluid, we concluded that there could be no divergence, but that a simple ascending current would be produced near the sides, with a descending one near the axis of the vessel. We chose a vessel of wood, as the worst conductor we could make,* for obvious reasons.

Plate X represents the apparatus, A B is a stage for philosophical experiments contrived by Professor S^r Gravesande, C is a cylindrical vessel of mahogany of the following dimensions; outside diameter 4.75 inches, outside height 2.75 inches, inside diameter 3.5 inches, inside depth 2.0 inches. In the upper edge, which was consequently 0.625 inches thick, there was a cylindrical excavation to the depth of 0.1 inches, and of the diameter of 4.5 inches. In the bottom of this vessel were fixed with sealing wax, two thermometers L and M, the one in the axis, and the other about 0.2 inches from the inside surface, and the bulb of each stood 0.75 inches higher than the inner surface, or bottom of the vessel; consequently the clear depth of the vessel from the upper edge to the thermometer, was 1.25

* A cylinder of glass 1 inch in diameter, and 0.25 inches thick, was placed on the horizontal cover of a vessel containing water kept boiling by a lamp, and a like cylinder of light dry mahogany was placed beside it at the same instant. A very minute cone of tallow stood on the center of each piece. That upon the glass fell down by fusion in one minute; that upon the wood in two. This method is much more accurate than that of Franklin with long rods described in Dr. Ingenhousz's Essays.

inches.

inches. A plate of steel I, four inches in diameter, and 0.3 inches thick, was used as the cover for the vessel; the remaining half inch of the excavation being left to receive the fluid, which would rise by expansion when heated. D is a tin vessel, of which the dimensions may be sufficiently known from the figure. It has a loose cover or valve at E, and is closed every where else. At H a branch proceeds, which ends in a cylinder F; and at G is a screen, to prevent the heat of the lamp K from affecting C; in addition to which, during the experiment, there was placed a pasteboard screen upon the stage near the letter K. It will now be easily seen that when the vessel C was correctly filled (except the excavation at top) with any fluid, and the steel cover laid in contact with the surface, and the vessel D partly filled with boiling water, and then placed in its situation here shewn, and, lastly, the spirit lamp K placed beneath the outer end of the branch H,—the temperature of 212° would be constantly applied to the steel, and the surface of the fluid accordingly heated. Whence if any heat proceeded downwards, it would be shewn by the thermometers; and if slow currents of different temperatures prevailed so as to produce the variation before stated, these thermometers would differ. It was thought that such a difference would prove the existence of currents, and the probability that the wooden vessel was the chief agent of the conduct; but it was not so clearly admitted that their agreement would prove the contrary position.

Brevity prevents my relating our arguments and views, and the nature of unpremeditated conversation, as well as other motives, forbid me the honour of mentioning the names of the philosophers to whom I refer in this discourse. I shall likewise, for the sake of conciseness, omit several experiments which were made with other apparatus of less simplicity, or leading to the same facts. One, however, with water I may cursorily notice. It was made in a glass vessel, with a cork bottom, nearly of the dimensions of the wooden vessel here described. During the time the boiling vessel E was kept in contact with the surface of the water, minute bubbles ascended slowly from the several parts of the water, which appeared to indicate that there was no perceptible current. A quantity of elastic fluid in bubbles occupied the surface of the liquid, and prevented its contact with the vessel. The thermometers were raised considerably, but slowly, as was the case in many other experiments with water; but as these require to be repeated, I omit the results.

Several trials were made with mercury and the apparatus in the Plate. The following was made with much care:

Pure distilled mercury was put into the vessel, and left undisturbed, with the steel cover upon it, for upwards of an hour. The vessel D was then partly filled with boiling water, and carefully placed on the steel cover, and the lamp K lighted. This was at thirty-two minutes past eight. The conclusion of the experiment was effected by lifting off the vessel D by the hand covered with a glove.

Time.

Time.	Inner Ther.	Outer Ther.	Time.	Inner Ther.	Outer Ther.	Time.	Inner Ther.	Outer Ther.
8 ^h 32 ^m	75 $\frac{1}{2}$ °	75 $\frac{1}{2}$ °	37 ^m	88°	88°	41 $\frac{1}{2}$	105	105
33	77	77	37 $\frac{1}{2}$	89	89	42 $\frac{1}{2}$	106	107
33 $\frac{1}{2}$	78	78	38	91	91	43	108	109 $\frac{1}{2}$
34	79	79 $\frac{1}{4}$	38 $\frac{3}{4}$	94	94	43 $\frac{3}{4}$	111	112
34 $\frac{1}{2}$	80	80 $\frac{1}{4}$	39	95	95	44 $\frac{1}{2}$	113	114
35	81	82	39 $\frac{3}{4}$	97	97	45	115	116
35 $\frac{1}{2}$	83	83 $\frac{1}{2}$	40	98	98	45 $\frac{3}{4}$	117	118
35 $\frac{3}{4}$	84	84 $\frac{3}{4}$	40 $\frac{1}{2}$	100	100	46	118	118 $\frac{3}{4}$
36 $\frac{1}{2}$	86	86 $\frac{1}{2}$	41	103	103	46 $\frac{1}{2}$	120	120 $\frac{1}{4}$

Another wooden vessel, exactly of the same dimensions, and fitting up, was then filled with olive oil, and left with its steel cover on for a short time, and then the boiler and the lamp were placed. It is to be observed, that the lamp was very sufficient to keep the water rapidly boiling over the water in the glass vessel; that it boiled but moderately over the mercury; and with much noise and tremulous action over the oil. This, and the difference of time employed in heating the different fluids, must shew either a difference of facility in circulation, or in receiving and giving, or in absolute conducting power in equal bulks. The mercury, it is seen, required seven minutes to raise the middle thermometer through twenty-one degrees, from 77° to 98°; but the oil required thirty-four minutes, or was nearly five times as long in conducting or conveying the same heat.

Table of the Experiment with Olive Oil.

Time.	Inner Ther.	Outer Ther.	Time.	Inner Ther.	Outer Ther.	Time.	Inner Ther.	Outer Ther.
9 ^h 7 ^m	72°	72°	33 $\frac{1}{2}$ ^m	78°	79°+	41 $\frac{3}{4}$ ^m	82 $\frac{3}{4}$ °	84°
16	73—	73+	34 $\frac{3}{4}$	79	80	44	84	85 $\frac{1}{2}$
22 $\frac{1}{2}$	74	74 $\frac{1}{8}$	37	80	81	56 $\frac{1}{2}$	90	92—
27	75 $\frac{1}{2}$	76	38	80 $\frac{3}{4}$	82	61	92	94
31	77	78	40 $\frac{1}{2}$	82	83 $\frac{1}{2}$	75	98	100

While this experiment was making, another boiler was kept in action upon a glass cylindrical vessel filled with olive oil. An observer carefully noticed the small opaque particles suspended in the fluid, but he could perceive no current during the continuance of thirty minutes.

On Light & Sound. (see pages 72, 81, 121)

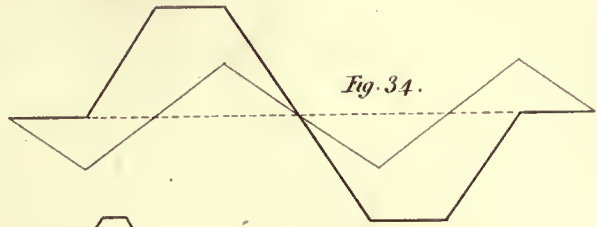
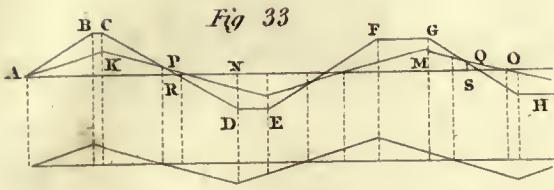


Fig. 35.

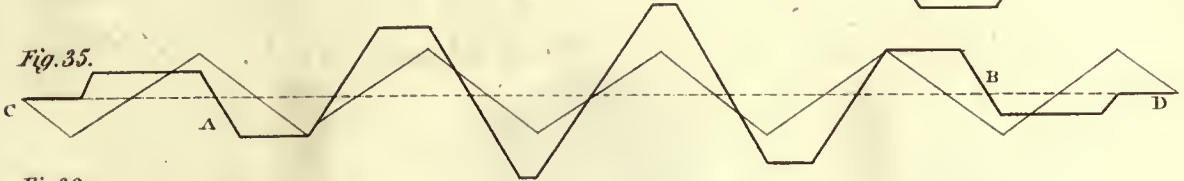


Fig. 36.



Fig. 37.



Fig. 38.

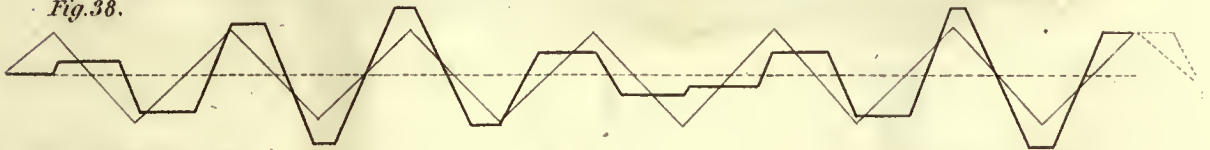


Fig. 29.

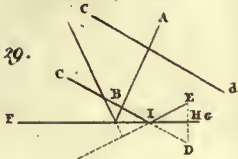


Fig. 39.

Fig. 30.

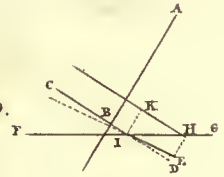


Fig. 40.

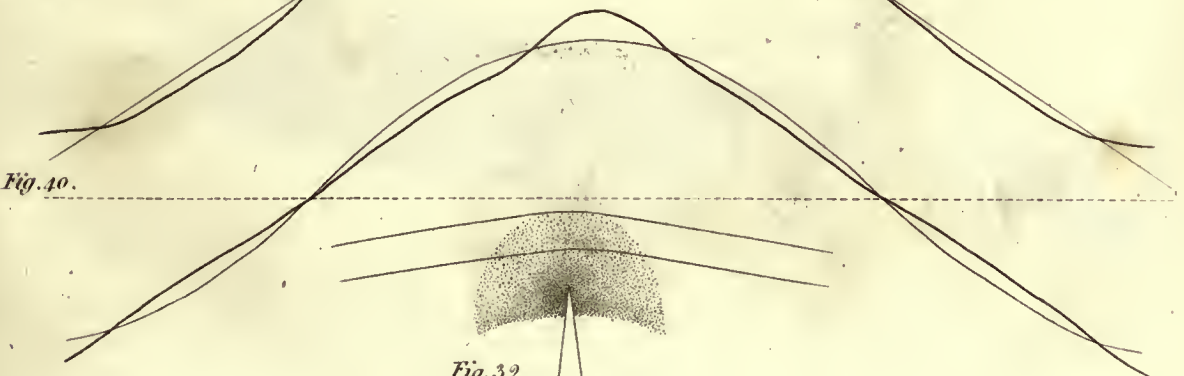
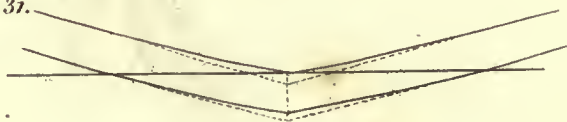


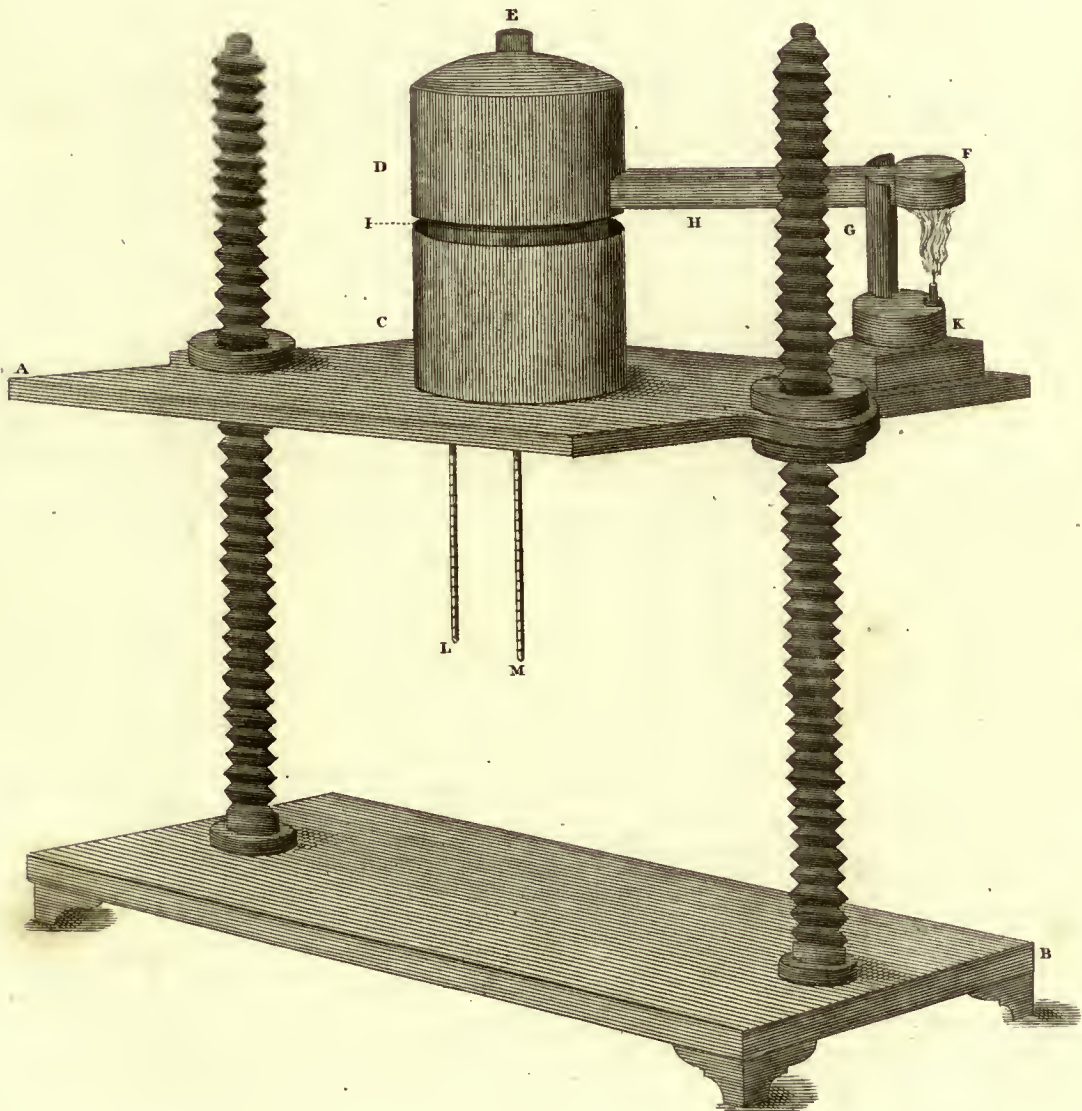
Fig. 32.

Fig. 31.





Transmission of heat thro' fluids downwards.





A
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AND
THE ARTS.

SEPTEMBER, 1801.

ARTICLE I.

Some Additional Observations on Hydrocarbonates, and the Gaseous Oxide of Carbon. By
WM. CRUICKSHANK, Woolwich.

IN a former paper on this subject the principal object I had in view was to point out, as briefly as possible, the difference between the common hydrocarbonates and the gaseous oxide of carbon; and to shew that the latter, hitherto unknown, forms an intermediate substance between the pure hydrocarbonates and carbonic acid, but not being possessed of acid properties, was therefore to be considered as a true oxide. I likewise endeavoured to prove that the presence of water was not essential to the production of this gas, but that it consisted simply of oxygen and carbon in the proportion of nearly two to one. From these facts I inferred that there could no longer be any difficulty in accounting, on the principles of the modern chemistry, for the inflammable gas generated in distilling a mixture of charcoal with any of the metallic oxides, or in exposing to a strong red heat a mixture of the carbonates of lime or barites, and iron filings, &c. For in the first instance the gas was formed by the union of the oxygen of the calx with the carbon, in consequence of which the metal was revived; and in the second case the carbonic acid itself was decomposed, being deprived of part of its oxygen by the metal, which thereby became to a certain degree oxidated.

In the course of experiments from which these conclusions were drawn, a number of curious facts with regard to the different hydrocarbonates, as well as the gaseous oxide, occurred; but these not being so immediately connected with the points to be ascertained, were, for the sake of brevity, omitted; I shall now, therefore, enumerate some of the more remarkable, as well as adduce several additional proofs of the nature and composition of the gaseous oxide of carbon.

The effects of the oxygenated muriatic acid upon these, and indeed upon all inflammable gases, are worthy of attention. If the pure oxygenated muriatic acid, in the form of gas, be mixed in certain proportions with any of these inflammable gases, and introduced into a bottle filled with and inverted over water, although no immediate action may be at first perceptible, yet in twenty-four hours a complete decomposition and change of principles will be found to have taken place, the products varying according to the nature of the inflammable gas employed, as will appear from the following experiments.

I introduced into a phial (with a glass stopper) filled with and inverted over water, one measure of pure hydrogen, and afterwards two measures of very pure oxygenated muriatic acid gas;* these nearly filled the bottle; the stopper was then introduced very tight under water. Before the stopper was introduced, a whitish cloud appeared in the mixture, but very little or no diminution could be perceived. The neck of the bottle being immersed under water, was suffered to remain for twenty-four hours. At the end of this period the water had risen a little in the bottle, owing to the imperfection of the stopper; but when this was withdrawn, the whole gas instantly disappeared, except about one-tenth of a measure, which was found to be azote, and must have originally been contained in the two measures of oxygenated muriatic acid gas. In this case the products were manifestly common muriatic acid and water; for the water in the phial contained common muriatic acid, but did not in the least smell of the oxygenated. This experiment was repeated with the same results, except that the residuary gas, on the admission of water, was rather less.

This fact furnishes us with an easy method of ascertaining the purity of hydrogen gas in most cases, and even when it may be mixed with other inflammable gases, as will presently appear.

I next tried the effects of this gas upon the pure hydrocarbonates. For this purpose I introduced into a bottle, filled with and inverted over water, one measure of pure hydrocarbonate (from camphor) well washed, mixed with two measures of the oxygenated muriatic acid gas. On the first contact of the oxygenated gas, a white cloud, and some diminution, was perceived; but this soon ceased; the stopper was then introduced, and the bottle suffered to remain inverted in water for twenty-four hours. At the end of this period the stopper was withdrawn under water, when the fluid instantly rose, and the

* This gas was always obtained by adding the common muriatic acid to the oxygenated muriate of potash. It should never be kept long before it is employed, as it very soon loses part of its oxygen, and becomes decomposed.

gas occupied only .43 parts of a measure, or a diminution of 2.57 measures had taken place. This residuary gas being agitated with lime water, .09 parts more were absorbed, evidently carbonic acid gas. The washed residuum was inflammable, and burned with a blue lambent flame, exactly similar to the gaseous oxides, but very different from that of the original hydrocarbonate.

This experiment was repeated with nearly the same results.

In this instance, however, the gas had been recently obtained from camphor, which I have always found made a considerable difference in the proportions of the products.* On the addition of the oxygenated muriatic acid gas to the hydrocarbonate, a diminution of no less than three-fourths of a measure immediately took place, and after standing for twenty-four hours, the water, on withdrawing the stopper, rose so high, that the remaining gas occupied the space of not more than .54 parts of a measure; hence the whole diminution must have been 2.46 measures. This residuary gas contained one-fifth of its bulk of carbonic acid, the remainder being inflammable. Two small measures of this inflammable gas, fired with two measures of oxygen in a jar, over mercury, were reduced to 2.5 measures; of this 1.7 were carbonic acid, and the remaining .8 or .9 oxygen gas; for it required very nearly two measures of nitrous gas to saturate them.

Hence we perceive that two measures of this gas, combined with about one of oxygen, produced no less than 1.7 measures of carbonic acid; a circumstance which proves it to have been the gaseous oxide of carbon.

Being determined to vary the experiments as much as possible, to prevent any mistake or oversight; I introduced into a bottle, filled with and inverted over mercury, two measures of hydrocarbonate from æther, and immediately after four measures of the oxygenated muriatic acid gas. These completely filled the bottle, which was stopped to prevent the acid gas from acting upon the mercury; its neck was then plunged under mercury, and kept down by a weight placed upon it.

After standing for twenty-four hours, the stopper was withdrawn under mercury, which instantly rose and filled two-thirds of the bottle; there was a little moisture deposited on its sides, which probably absorbed the whole of the common muriatic acid gas produced. Lime water in excess being now added; one-third was found to be carbonic acid, and the remainder inflammable.

* It might be supposed that this difference arises from the absorption of carbonic acid gas; but this is by no means the case, for the proportion of this gas in the hydrocarbonates from æther, or camphor, is exceedingly small, never exceeding one-tenth, and frequently much less. Besides, I found, from direct experiment, that the unwashed hydrocarbonate required more oxygen to saturate it, than the washed, the reverse of which should have been the case, had the difference depended upon carbonic acid. This variation in the results may be thus stated:—One measure of unwashed hydrocarbonate requires 1.9 of oxygen to saturate it, the products being water, and 1.4 measures of carbonic acid. But one measure of the washed gas requires only 1.65 measures of oxygen, and produces about 1.15 measures of carbonic acid with water.

It appears to me, therefore, that something like a vapour diffused through the gas, which may be separated by washing, or deposited on standing, must be the cause of this change in their properties.

Two small measures* of this washed gas, fired over mercury with one of oxygen, were reduced to 1.6 measures; the whole of which was carbonic acid, except one-fourth of a measure which contained oxygen, with a little azote.

This experiment, therefore, as well as the former, after making a deduction for the azotic gas, clearly shews that the residuary inflammable gas must have been the gaseous oxide of carbon.

The results obtained in all these experiments with the pure hydrocarbonates and oxygenated muriatic acid gas, are very easily understood; for it is evident that the oxygenated acid must give out its excess of oxygen to the carbon and hydrogen of the inflammable gas from a superior affinity; in consequence of which we have no less than four new compounds, viz. common muriatic acid, water, carbonic acid, and the gaseous oxide of carbon. Thus, then, we see that the gaseous oxide may be obtained without the aid of metals, their calces, or even the carbonic acid; and this simple mode of formation appears to me to demonstrate its nature and constituent principles beyond all possibility of doubt. This will be still further confirmed by the following experiment:

1. One measure of hydrocarbonate from camphor, mixed with 3.8 measures of oxygenated muriatic acid gas, were introduced into a bottle, which they completely filled; this was closely stopped, and inverted over mercury. After standing for twenty-four hours, the stopper was withdrawn under the mercury, which in consequence immediately ascended and occupied two-thirds of the bottle. The gas acted upon the surface of the metal, and retained a little of its natural greenish yellow colour; it was not, therefore, entirely decomposed. A little distilled water being introduced, a further absorption took place, and the residuary gas, amounting only to .8 of a measure, was entirely absorbed by lime water, except a bubble, too small to examine, or even to estimate. Now, by comparing this with the former experiments, we perceive that when one part of the inflammable gas is mixed with only two of the oxygenated muriatic acid gas, we have both carbonic acid and gaseous oxide formed, because the excess of oxygen in the oxygenated acid is not sufficient to convert the whole into carbonic acid; but when one part of the inflammable is mixed with nearly four of the oxygenated, then the whole is converted into carbonic acid and water, the excess of oxygen being sufficient for this purpose. The proportion of oxygen in the gaseous oxide may likewise be estimated from these experiments.

The effects of the oxygenated muriatic acid on the other hydrocarbonates, were very nearly similar.

When first mixed, a small diminution in most instances took place, but after standing for twenty-four hours, the oxygenated acid was completely decomposed, in consequence of which the hydrocarbonate was converted into water and carbonic acid gas, and there remained only a very small proportion of inflammable gas, which had all the properties of the gaseous oxide.

* Two kinds of measures were employed in these experiments, the contents of the large one being six times that of the small.

Being anxious to see the effect of this gas on the gaseous oxide itself, I mixed two measures of this oxygenated gas with two of the gaseous oxides well washed, and introduced them as usual into a bottle inverted over water. After standing for twenty-four hours, the stopper was withdrawn, and the water instantly rose so high, that the remaining gas occupied the space of only 1.8 measures, or a diminution of 2.2 had taken place. This, when agitated with lime water, was further reduced to $\frac{1}{6}$ of a measure, and the residuary gas was still slightly inflammable.

This experiment was repeated with only this variation, that the bottle was completely filled, and had its neck plunged under mercury: the proportions of the gases were two measures gaseous oxide, to $2\frac{2}{3}$ measures oxidated muriatic acid gas. After remaining for twenty-four hours, the stopper was withdrawn under water, when the fluid instantly rose and occupied two-thirds of the bottle. This residuum being agitated with lime water, the whole was absorbed, except one-sixth of a measure, which was found to be azote. Hence we perceive that any quantity of gaseous oxide may be converted into carbonic acid by mixing it with the oxygenated muriatic acid gas, just as the oxide of azote or nitrous gas are converted into nitrous acid by the same means.

There is another gaseous substance, or rather vapour, which is powerfully acted upon by this super-oxygenated gas, viz. the vapour of æther; if we fill a bottle of the capacity of three or four pints with the pure oxygenated muriatic acid gas, taking care to expel the water as completely as possible, and then throw into it about a drachm or half a drachm of good æther, covering its mouth immediately with a piece of light wood or paper; in a few seconds white vapour will be perceived moving circularly in the bottle; this will be soon followed by an explosion, accompanied with flame; at the same time a very considerable quantity of carbon will be deposited, and the bottle will be found to contain carbonic acid gas.*

If we employ alcohol instead of æther, a similar effect will be sometimes produced, but more time is necessary, and along with the charcoal and carbonic acid there is likewise a little æther formed.

We shall next consider the effects produced on these inflammable gases, by mixing them with the oxygenated muriatic gas, and setting fire to the mixture by the electric spark. In

* Dr. Priestley was the first who discovered that æther, agitated with any species of gas, greatly increased its volume, and in most cases doubled it. He likewise found that although this vapour was not condensible by the ordinary degrees of cold, yet water readily absorbed it. Having agitated some oxygen gas with a little æther, I found that its bulk was exactly doubled. In this state the gas does not explode; but if one part of this mixture be added to three parts of oxygen, an ignited body, or the electric spark, then produces a dreadful explosion, the products of which I have found to be water, with $2\frac{1}{3}$ parts of carbonic acid gas. Hence it would appear that one part of this vapour requires about seven of oxygen to saturate it, or more correctly 6.8 the products being water, with 4.6 parts carbonic acid. According to this experiment the proportion of carbon to hydrogen in the vapour of æther, or æther itself, should be as 5 to 1 nearly; and in alcohol, from similar trials, it appeared to be as 8 or 9 to 1.

many

many of these trials the results differed more from the former, where mixture alone was employed, than could *a priori* have been supposed, as will appear from what follows.

Three measures of pure hydrogen were introduced into a strong glass jar over mercury, and four measures of the ox. muriatic acid gas added, the electric spark was immediately passed through the mixture, to prevent, as far as possible, the absorption of the gas by the mercury; after the explosion, which was feeble, the gas occupied the space of .75 of a measure: water being admitted the mercury still rose higher, so that there remained only .2 of a measure. Finding that in this experiment the oxygenated gas was in excess, the proportions were varied as follows:

Four measures of pure hydrogenous gas were fired with four of the ox. muriatic gas; the residuum amounted only to half a measure, but this contained hydrogen. Hence by comparing these experiments together, it would appear that three measures of hydrogenous require three and a half of oxygenated muriatic acid gas to saturate them; the products being water and common muriatic acid. From the same experiments we can likewise deduce the excess of oxygen in the oxygenated muriatic acid, or the proportion of oxygen necessary to convert the common muriatic acid into the oxygenated; for according to the above proportions, two parts of hydrogen gas require 2.3 of the oxygenated, to saturate them; now two parts of hydrogen require exactly one of oxygen (estimating by volume) to produce the same effect. Hence every 2.3 parts of the ox. muriatic acid contain one part of oxygen, or about half its bulk more than the same muriatic radical when in the form of common muriatic acid gas; this fact may be found useful in many chemical investigations where the super-oxygenated acid is employed. In this way too we have a ready way of determining the purity of the hydrogen, for if these gases perfectly pure, be fired together over mercury in the above proportions, the whole will disappear; but if the hydrogen should be contaminated with azote, or some inflammable gas, there will then be a residuum consisting either of azote, or carbonic acid gas, or both.

The next experiments were made with the pure hydrocarbonates. Here I found, when two parts of pure hydrocarbonate, recently procured from camphor, were fired over mercury by the electric spark, with four of the ox. muriatic acid, that a great diminution, accompanied with a considerable deposition of charcoal took place; the residuary gas generally amounted to 1.2 or 1.5 measures, and of this lime water absorbed about 1. or 1.2 measures, the remainder being inflammable. In this instance the hydrocarbonate did not yield above half the proportion of carbonic acid which it ought to have done, but this is sufficiently accounted for, from the copious deposition of charcoal.

When hydrocarbonate from æther was employed, the results were very nearly the same, and charcoal was more generally deposited. Finding that in these proportions the quantity of oxygen contained in the ox. muriatic gas was insufficient to saturate the carbon and hydrogen of the hydrocarbonates, I next fired one measure of the latter with four of the former; after the explosion, the gas stood at two measures; a very little distilled
water

water being added, the mercury rose to 1.6, and lime water absorbed the whole of the remainder, except .2 of a measure which was inflammable.

In another experiment, no less than five measures of the oxygenated gas were mixed with one of the hydrocarbonate, but still at the end of the process there remained a very small proportion of inflammable gas, and the whole of the oxygenated was not decomposed. In these instances, where we use but a small proportion of the hydrocarbonate, the explosion is very feeble, and there is seldom or never any deposition of charcoal, in consequence of which there is a more copious production of carbonic acid gas, as may be readily seen by comparing the experiments together. Indeed in the last one, the same proportion of carbonic acid was obtained, as if the gas had been exploded or saturated with oxygen; for one measure of pure hydrocarbonate requires very nearly two measures of oxygen to saturate it the products being water, and 1.3 or 1.4 measures carbonic acid gas. Now in the experiment with the oxygenated gas, one measure of the hydrocarbonate fired with five of this gas, produced just 1.3 measures carbonic acid, with water, &c. indeed since five measures of the ox. muriatic acid gas contain an excess of oxygen equivalent to two measures, or a very little more, this result ought to have been expected.

There was nothing very remarkable observed in exploding the impure hydrocarbonates with this gas; all of them yielded carbonic acid gas and water, but in none of them did I perceive any sensible deposition of charcoal, except once from the gas obtained from the decomposition of alcohol. Indeed the pure hydrocarbonates themselves, in a great measure, lose this property after having been washed, or kept over water for some time; that from æther retains it the longest. Several attempts were made at different times to explode mixtures of the ox. muriatic acid gas, and the gaseous oxide of carbon, but without success. On passing the spark a little whitish vapour was perceived round the metallic points, but this was not accompanied by any detonation, and even after a number of strong sparks had been taken in this way, it was found on the admission of lime water, which immediately took up the oxygenated gas; that the gaseous oxide had undergone no sensible diminution or change. This circumstance was rather unexpected; it furnishes us, however, with at least one criterion for distinguishing this from other inflammable gases.

On comparing the above experiments with the results obtained from firing the hydrocarbonates with different proportions of oxygen, some observations not unworthy of attention may be made. We know that when any of the pure hydrocarbonates are fired with a small proportion of oxygen, a little carbonic acid is produced, and the whole volume of gas instead of being diminished is considerably increased; this increased gas contains no disengaged oxygen, but is nevertheless very different from the original hydrocarbonate, requiring only bulk for bulk, about one fourth of the quantity of oxygen to saturate it*; no charcoal is ever disengaged. But when we explode the same hydrocarbonate with about twice its bulk of the ox. muriatic acid gas, we have a great diminution, accompanied

* It does not appear to be gaseous oxide, which I believe cannot be produced this way,

with the separation of much charcoal, and only a small proportion of carbonic acid generated; the remaining gas, which is inflammable, and in small quantity, appears to be gaseous oxide. Thus then we find, that by combining the same inflammable gas with equal quantities of oxygen under different circumstances or combinations, very different effects may be produced.

On a former occasion I remarked, that from the great facility with which the carbonic acid was decomposed, when a mixture of clear iron filings and chalk were heated together in a retort, it would appear that this acid at a high temperature might be decomposed, at least partially, by any substance which had a considerable affinity for oxygen; and that this was the only method of obtaining the gaseous oxide in a state of purity. As this is a circumstance of considerable importance, the following experiments are given in further confirmation of it:

About an ounce of chalk, which had been previously exposed for about ten minutes to a low red heat, was mixed with an ounce of very bright zinc filings, these were introduced into a coated glass retort, and exposed to a heat gradually increased. A little carbonic acid came over at first, but this was soon mixed with a large proportion of the gaseous oxide, and by the time the contents of the retort were thoroughly red, nothing but pure inflammable gas was disengaged, and in prodigious quantity. It would have been troublesome, and in fact unnecessary, to have collected the whole, but portions of it were examined at different periods of the process, and found to continue perfectly pure, and without any sensible mixture of carbonic acid gas. Indeed, before I made this experiment, I hardly conceived it possible, that so complete a decomposition could be obtained in so simple a way. Before the gas had entirely ceased to come over, the retort was removed, and its contents examined. In the neck of the retort there was found a quantity of the oxide of zinc in the form of flowers, lining its surface to a considerable extent: a little below this, and close to the belly of the retort, there adhered a little metallic zinc which had sublimed, but was very much tarnished; what remained in the belly consisted of the zinc destitute of all metallic lustre, and mixed with an imperfect lime. The pieces of zinc, or rather oxide, crumbled between the fingers into a greyish powder, and seemed to have entirely lost their tenacity. It is unnecessary to add, that the gas obtained was found when fired with oxygen, &c. to have all the properties of the purest gaseous oxide of carbon. The above experiment shews in the most satisfactory manner, how the oxygen abstracted from the carbonic acid is disposed of, for in the processes with the iron filings this was not so manifest as might have been expected.

A mixture of very dry chalk and clean filings of tin were next exposed to heat in a coated glass retort; the first products collected consisted of carbonic acid gas, and the gaseous oxide in the proportion of 5 to 6. The second or middle portion contained two thirds of its bulk of gaseous oxide, and what came over afterwards consisted of one part carbonic acid, and three parts gaseous oxide. The quantity in all amounted to several gallons. After the process the tin was found partly oxidated, and

partly diffused through the chalk in small metallic globules. The decomposition of the acid in this experiment was very imperfect, when compared with that in the former, owing no doubt to the different affinities of the metals for oxygen.

Although there could be no doubt, that in all these processes with the carbonate of lime and the metals, the carbonic acid must have been decomposed in its nascent state, yet I conceived that it might give still an additional satisfaction to shew, that the same substance may be readily decomposed even when in the state of gas. This was accomplished in two ways.

Into a straight iron retort with a cylindrical belly, a quantity of dry and very pure sand was introduced, and above this a stratum of chalk which had been very carefully dried; more sand was then introduced, and rammed down, until it filled up no less than three inches of the cavity above the chalk, and upon this was placed a quantity of very clean iron filings, the whole occupying about two thirds of the belly. The retort was then fixed in a furnace, and made red hot as soon as possible; the first gas which came over was chiefly carbonic acid; the second portion amounting to a gallon, consisted of carbonic acid gas one part, and gaseous oxide three parts; the third portion being upwards of two gallons by measure, consisted of carbonic acid two parts, and gaseous oxide seven parts; and the last portions examined, contained a somewhat greater proportion of carbonic acid gas. The gaseous oxide was found to be of the purest kind. In this experiment the carbonic acid was not brought into contact with the iron in its nascent state, but after it had assumed its gaseous form, and passed through a quantity of sand. The iron filings too being unmixed with the chalk, shewed evident marks of oxidation, a considerable portion of them being converted into the state of grey oxide.

The other method by which I decomposed this acid in its gaseous form, was by making it pass through a red hot iron tube filled with iron wires. In this experiment, a bladder mounted with a stop cock, and containing a quantity of carbonic acid gas, was attached to one end of the tube, so as to be air tight at the joining; the tube was then passed through a portable furnace, and an empty bladder mounted in the same manner fixed to its other extremity; heat was then applied to the middle of the tube until it became red, taking care at the same time to keep its extremities cold, by means of wet cloths wrapped round them. When the tube was sufficiently hot, the gas was made to pass slowly through it from the full bladder to the empty one, and then back again from the latter to the former. This was repeated ten times, when a little of the gas left in one of the bladders was examined, which by means of the stop cocks could be managed at pleasure. This gas was found to consist of one part inflammable gas, and two parts carbonic acid; the bladder was then re-applied, and the gas forced backwards and forwards ten times as before; on re-examination it was now found to consist of inflammable gas, and carbonic acid gas in the proportion of 11 to 5; but by continuing the process a little longer, no less than three fourths of the original carbonic acid gas was converted into an inflammable gas, and which on examination was found to be the true gaseous oxide of carbon; the wires after the

tube had cooled were examined, and found covered in several places with a beautiful shining crust of grey oxide, which readily crumbled and fell off on bending the wire. The carbonic acid employed in this experiment was procured from carbonate of soda by means of the dilute sulphuric acid, and amounted to 64 ounce measures; during the process very little diminution or increase of bulk could be perceived, which is readily accounted for, from the very trifling diminution the gaseous oxide undergoes when converted into carbonic acid gas.

From the various facts which have been now enumerated, added to those already known, there can no longer remain any doubt, that the carbonic acid, under certain elevations of temperature, may be readily decomposed by any fixed substance, having a considerable affinity for oxygen; and that with regard to the metals, those succeed the best which have the greatest attraction for acids or oxygen, as is remarkably exemplified in zinc.

Upon the whole then I think we may conclude, that there are but two distinct combinations of carbon and oxygen capable of assuming and maintaining the form of gas; for although the gaseous oxide may be obtained in a great variety of ways by direct composition, as well as the decomposition of the carbonic acid, yet when pure it is exactly the same substance, having the same specific gravity, requiring the same quantity of oxygen to saturate it, and producing uniformly the same proportion of carbonic acid. I remarked indeed formerly, that the gas obtained from charcoal (however well dried) mixed with the metallic oxides, always yielded a little water, when burned in a receiver containing common air or oxygen gas, and this impurity was ascribed to hydrogen contained in the charcoal itself. Now that this may really be the case, appears to be in some degree confirmed by the following experiments.

A quantity of charcoal obtained by distillation in iron cylinders, was introduced into a retort without being moistened. A great quantity of gas was in all produced, what came over about the middle of the process consisted of carbonic acid and hydrocarbonate in the proportion of 1 to 18. * Two measures of this washed hydrocarbonate required 1.6 measures of oxygen to saturate them, the products being one measure carbonic acid gas with water. Another portion of the same charcoal, which had for some time been exposed to a red heat in a covered crucible, was introduced into a coated glass retort, and a strong heat being applied, a considerable quantity of gas came over. This gas, except at the very commencement, scarcely contained a vestige of carbonic acid; two measures of it required 1.25 of oxygen gas to saturate them, the products being water with only .55 parts of a measure of carbonic acid. Now by comparing these experiments together, it would appear that the gas from the charcoal which had been so completely deprived of its moisture, contained bulk for bulk more hydrogen, and less carbon than the gas from that which had not been dried; this being the case, it can hardly be supposed that the hydrogen proceeded from the decomposition of water, but must have somehow entered into the composition of the charcoal itself. The total absence of carbonic acid may be considered as a further

* The gas obtained in this way is extremely variable, and towards the beginning of the process I have sometimes met with traces of the gaseous oxide.

proof of this. Another argument in favour of this supposition may likewise be drawn from the uniform loss of weight which has been observed in burning charcoal in pure air or oxygen gas; that is to say, the weight of the carbonic acid produced has never been found equal to that of the oxygen and charcoal consumed; this deficiency I make no doubt, has arisen from the production of water which was afterwards held in solution by the carbonic acid gas, and could not therefore appear in its natural state. That most gases contain a large proportion of water at the common temperature of the atmosphere can hardly be doubted, although it may be very difficult to estimate the quantity, for the driest gases exposed in close bottles to freezing mixtures, will be found to deposit a considerable quantity of moisture, which will again disappear on raising the temperature. It is not improbable, therefore, that water may be frequently produced without our being able to detect it. It is no doubt in a great measure from this impurity in the charcoal, that all the gases obtained from mixtures containing it, are so uncertain and variable in their properties.

Woolwich, Aug. 19, 1801.

II.

Short Notice concerning the Properties and external Characters of some new Fossils from Sweden and Norway; together with some Chemical Remarks upon the same. By Mr. D'ANDRADA. In a Letter to Mr. Beyer, Master of the Mines at Schneeberg.

(Concluded from page 196.)

IX. WERNERITE.

ITS colour is a medium between the pistaccia-green and Isabella-yellow, of different shades of yellow. Inwardly its lustre is of the fatty kind, approaching to that of the mother-of-pearl, sometimes in a high degree resplendent, and sometimes a little chatoyant, but without any distinct play of colour. In small fragments it is very transparent; and its specific gravity is 3.6063. It cuts glass, but gives little fire with steel, and can be scratched by the common feld-spar. Its texture is somewhat curvilinearly foliated, and when the lamellæ are considerably curvilinear, the fracture appears chatoyant. The laminae seem to cross each other twice in an oblique direction. The cross fracture is uneven and fine splintery; and its fragments are splintery, with sharp edges and pointed. Wernerite occurs in masses and crystallized. The crystals are low, hexahedral columns, with tetrahedral terminations, having their terminating faces imposed on the lateral edges. In those specimens, which are found in lumps or masses, the form of aggregation is large and coarse granular, and very much concreted. This stone readily frothes upon charcoal before the blow-pipe, and its edges exhibit an opaque, white, imperfect enamel. It occurs in the iron-mines Northo and Ulrica, in the Arendal territory in Norway, and also in Campolongo, in the Lewindale, in Swisserland. This fossil bears great resemblance to the adamantite spar in its colour and lustre.

X. PETALITE.

The colour most frequently occurring is reddish, and in some instances greyish-white. Its internal lustre is ordinary and glittering, now and then with a little splendor, and in that case of a faint appearance of mother-of-pearl. The edges are a little transparent. Specific gravity rather above 2.620. It cuts glass and is itself scratched by feld-spar. It hardly strikes fire with steel. It occurs in lumps of a fine and also of a rather coarse aggregation. Its texture is foliated or scaly. The lamellæ are very minute, throughout strongly concreted with each other. Their crossing is but single. The fragments of the cross fracture are angular, amorphous, and not very sharp in the edges. It is broken with great facility, and easily reduced by grinding into a subtle, white, rough, and dry powder. When one piece is rubbed against another, it emits a faint smell resembling quartz. When treated alone with the blow-pipe it is infusible, without change of colour or lustre. With borax it produces a white, transparent, vitreous globule, and with microcosmic salt a yellowish-white pearly glass, full of fine air-bubbles. With nitric acid it does not effervesce, whether in the state of grains or of powder, but a portion is gradually dissolved by that acid. Petalite is found near Utoen, Sala, and Fingrufan, near Nyakoperberg, in Sweden.

XI. CHRYOLITE.

Colour snow-white. Lustre faint, like a weak splendor of mother-of-pearl. Very transparent. Specific gravity 2.9698. It scratches calcareous spar, but is itself scratched by fluor-spar. It may be readily broken in pieces, and is rather soft. It yields a very subtle, white powder, of a soft feel, which, if moistened with water, becomes transparent. This fossil feels dry, and is cold, like feld-spar. Its texture thick and broad foliated, with an irregular roughness, like water that has been suddenly congealed to ice. Its laminae are straight, and their crossing threefold. The parts separated by splitting, when entire, are of a cubical form. The aggregation of its integrant parts is such, that the juncture of two is always covered by a third, super-imposed like bricks, which originates from the circumstance, that two crossings are straight and of the broad foliated kind, while the third is partly laminated, partly broken, partly uneven. Before the blow-pipe chryolite fuses even before ignition, like ice melting without effervescence; and it yields a snowy-white opaque pearly mass, which by a stronger heat becomes rough, full of bubbles, and depressed in the middle, being also caustic in that state when put on the tongue, and possessed of a taste similar to that of borax. When heated with borax, this fossil is fused to a pellucid glass, which, however, upon cooling is opaque and white. When fused with pot-ash in a silver crucible, it turns to a white porcelanic mass; which, when dissolved in distilled water, and precipitated by means of nitric acid, yields a white, transparent pasty mass, which after desiccation may be fused again in the same manner as the crude fossil. In the nitric and

muriatic

muratic acid it is insoluble. With very concentrated sulphuric acid it strongly effervesces, emitting then whiteish, gaseous vapours, which attack glass. This peculiar fossil consists of alumine, fluoric acid, and a little pot-ash. It occurs in Greenland, but its native particular spot is not yet known; though it appears to form strata.

XII. SCAPOLITE.

The colour of this stone is yellowish and greyish-white, and also smoky-grey. Its external lustre vitreous, from the gloss of wax to a brighter polish, but its internal lustre has little brilliancy. This stone is more or less transparent on the edges, but it is entirely opaque when decayed. Specific gravity from 3.680 to 3.780. It admits of being scratched with a knife, but itself scratches glass. Its streak is white. The crystals are nearly rectangular quadrilateral columns (having angles from 85° to 95°), with very slightly truncated lateral edges, without pointed terminations. These crystals are very minute, and in that case almost needle-shaped; seldom large, with transverse shootings, and longitudinally striated. The small and very small ones are usually concreted cross ways with each other in regular clusters (*drusen*) and prismatic; but the larger are totally bedded in the matrix. Their texture is lamellar, but in the larger crystals it approaches to the striated. When split a visible diagonal crossing is perceived lengthways, and a second one less perceptible which is longitudinal and oblique-angular. The cross fracture of scapolite is uneven, and the fragments of this fracture are prismatic (bar-like). It may be easily broken, and is not electric. Before the blow-pipe it readily fuses, with frothing, into a white resplendent enamel. It is found in the iron-mines near Arendal, in Norway.

III.

Experiments and Observations on the Properties of Wind Instruments, consisting of a single Pipe or Channel; with Improvements in their Construction. By Mr. WILLIAM CLOSE.

To Mr. NICHOLSON.

SIR,

ON every wind instrument of music, which has the intervals of its scale regulated by holes for the fingers of the musician, it is impracticable to transpose the key of an air half a tone, without altogether changing the mode of fingering. If no alteration can be made in the length of the instrument, to perform a quick succession of chromatic intervals is not easy; but when some of the chromatic notes are assumed for the key of an air, the fingering is attended with so many difficulties, that the melody can only be well executed by a performer of transcendent abilities.

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The chromatic semitones are generally introduced into the natural scale of each of these instruments by the assistance of several fingers. If they could be all conducted by one finger; or if all the notes of the scale could be raised half a tone by opening only one aperture, a succession of semitonic intervals could be attended with little difficulty. It would be easy to introduce any number of sharp sounds from another key, and as easy to play in the modes of C sharp, D sharp, F sharp, and G sharp with a greater third, as of C, D, F, and G; of G sharp, D sharp, and A sharp, with a minor third, as in the easy keys G, D, and A; because the scale in each key, with all its sounds made sharp; would be fingered in the manner as previous to its transposition.

The pitch of an instrument, indeed, may be raised by contracting its length, but this method can be of little use, except for making it accompany other instruments. It cannot be used in modulation to lessen the difficulties of fingering, as the melody must be suspended before it can be adopted.

Among some experiments which I have made with a view to improve the structure of certain wind instruments, I have endeavoured to realise a project for a very easy method of introducing the chromatic semitones into the natural scale, and of sharpening any number of the diatonic notes at pleasure: in the course of the present letter I shall communicate the method of doing this, and describe the principal instruments which have been constructed in the course of these experimental researches.

In describing the instruments I shall follow the order in which I have arranged the drawings.

Early in the year 1799, I had a large flute constructed of pewter, in the shape represented by Fig. 1, Plate XI. which is still in my possession. Its inside is a winding cavity or serpentine tube, into which are made seven holes at very convenient distances for the fingers commonly used in performing the scale of C major on the German flute. When all these apertures are closed, the lowest sound is in unison with the note represented by the line which is marked F by the bass clef, or it is one-fifth lower than the lowest sound of a German flute, which has three keys for the little finger of the right hand.

Fig. 2 represents an horizontal section of the tube or cavity, at any height between its higher and lower sides, on a scale of one-fifth of the real size. The inside of the tube is three quarters of an inch broad, and one inch deep.

That part of the instrument which contains the aperture for producing the sound, is laid over three turns of the tube, and is represented in Fig. 3, which is a vertical section of this part of the instrument, to shew the depth of the sides, and the construction of the tube near the sound hole. A sudden dilatation in this part is avoided, by the insertion of a piece of metal, which produces the gradual turn, represented by the dotted line: but none of the corners in any other part are there cut off.

These

These pieces or plates of metal in this construction of instruments of this kind, are bended and laid together in the manner delineated in Fig. 4, which nearly represents a cross vertical section of the instrument between the sound hole and the apertures for the fingers.

The length of the tube, and the places for the holes, were made to correspond with the proportions of an instrument that was straight, by measuring a line that was represented as drawn in the middle of the tube on a plan similar to that delineated in Fig. 2, but of the real size. The line took a semicircular turn at the end of every partition, and described the half of that curve at five other places. The empty corners were all disregarded in the calculation.

The holes for the fingers are marked 1, 2, 3, 4, 5, 6, 7, in Fig. 2: the hole 8 is covered with a valve or key, which is depressed by the little finger when the instrument is required to sound G sharp; at all other times, when the assistance of the little finger is requisite to steady the instrument, it is put down upon the seventh hole instead of being laid upon the key, as is commonly practised in playing the German flute.

That all sounds of the same name should be perfect octaves, is one of the first requisites in the scale of every instrument. It was expected that the angles or dilated parts in the anfractuous structure of this flute would render the octaves too flat, but when the seven lowest sounds were tuned by adjusting the sizes of the holes there was very little imperfection in each corresponding sound of the second octave. D and E, answering to A and B in the higher octave of the German flute, are those that are a little defective: they are too flat when the seventh hole is open, but when it is closed they are made true. Closing this aperture does not affect D and E in the lower octave.

An instrument of this kind should be made light, as it is generally supported in an horizontal position when applied to the lips, and the chief of its weight rests upon the thumb of the left hand. The form may be used for large instruments when it is requisite to contract the length, and bring the holes for the fingers into a convenient compass.

A flute of the same pitch with the German flute was constructed of the shape of Fig. 1, but the second octave of its scale was very defective, even when every precaution had been taken to fill up all the corners.

A small straight flute, which is about four tenths of an inch in diameter, thirteen inches in length from where it is filled with the cork, with seven holes for the fingers, and the nearest of these to the sound hole about five inches distant from its centre, will perform a scale of three octaves and a full tone, and will sound B and either of its octaves when only the first hole is closed. The compass of the scale of a common fife with six holes is two octaves and a sixth. The extension of the scale by a fife of these dimensions beyond that of the common one is owing to the additional piece of tube beyond the seventh hole. As performers on the fife generally use the acute sounds of the instrument, it will be of advantage to make this addition, even when the seventh hole cannot be closed by the little finger: it will extend the scale, and make some of the acute sounds easier to be taken in succession, by rendering the mode of fingering more simple.

I had once a very small metallic flute or fife which would play any two notes in the first and second octave which were octaves to each other, both together, and when it was rightly blown they were perfectly in tune. The sound hole was elliptical, and rather wide, and its edges were sharp. The holes for the fingers were wide, and the sides of the pipe were thin.

I shall now proceed to explain the method of transposing the key of an air on the German flute without any material alteration in the mode of fingering.

If one end of a round pipe, which is one-fifth of an inch in diameter, and two inches and an half long, be inserted into the side of a German flute, about one inch and three quarters nearer the sound hole than the aperture, which is usually closed by the foremost finger of the left hand, all the sounds in the lowest octave from C to B will be raised half a tone while both its ends are open, and all, or any of them, may be restored to the original pitch by only closing its exterior orifice.

If the end of another pipe, which is rather wider than the former, and one inch and an half long, be inserted into the flute between, and equally distant from, the fourth and fifth holes from the sound hole, these sounds of the second octave C, D, E, will be raised nearly half a tone; when C is produced, only the second hole from the sound hole should be open.

If the end of a third pipe, which is one quarter of an inch in diameter, and one inch long, be inserted into the flute at the same distance from the sound hole as the nearest aperture but one, all the notes in the second octave between C and C will be raised half a tone, when all the three pipes are open.

To reduce this to practice: Let the first pipe be so laid by the side of the flute that the end which opens into the external air may be closed by the thumb of the left hand; and let the other two pipes be so disposed that their ends may be closed by two valves or keys, which may be both opened at once by either the thumb or the little finger of the left hand.

Fig. 5 represents a German flute of the common size, which is chiefly constructed of tin and copper, and has the three additional pipes. A section of it is delineated in Fig. 6. The places of the holes for the fingers are there numbered 1, 2, 3, 4, 5, 6, 7. There is a turn in the tube between the sixth and seventh holes, in order that the seventh may be closed by the little finger of the right hand instead of a key. The three pipes are arranged in the order that has been recommended. That for sharpening the notes of the lower octave is closed by the thumb, and the other two by keys, which are delineated in Fig. 5.

The flute is rather straighter at the turn than in any other part, when it was not contracted between the sixth and seventh holes; some of the sounds in the second octave, particularly F sharp and G, were put out of tune when the seventh hole was closed.

When the above-mentioned pipes are added to flutes made of wood, they may either be let into the material, or formed by making grooves in the side of the instrument after it is lined with a metallic tube: these grooves may then be covered with pieces of metal, to which may be fixed the keys.

By the assistance of these pipes it is very easy to perform a chromatic scale, or at pleasure to transpose the key of an air half a tone. The truth of the intervals in the key thus assumed depends very much upon the width of the pipes. Those of the flute, delineated in Fig. 5 and 6, would be pretty true; but one or two octave notes of its original scale are too sharp, and this imperfection is transmitted to the transposed scale.

To determine whether the original sounds of the second octave could be raised by closing the interior ends of these pipes, or (which would answer the same purpose in diminishing the internal capacity of the instrument) by filling them with water, I made the following experiment:

The three pipes were inserted into that side of the flute opposite the holes for the fingers, and nearly at right angles with the tube. The lower end of each was then just immersed in water contained in a small vessel, and while the flute was held in a position nearly horizontal, sounding each of the notes C, D, E, F, G, A, B, of the second octave, and C, D, E, of the third, the vessels were raised so high, that the water would very nearly fill to the higher end of each pipe. No variation in any of these sounds C, D, E, F, G, was perceptible: but the pitch of the five higher, A, B, C, D, E, rose a little with the ascent of the water. The variation, however, was so slight as to be no objection to the use of the pipes.

When an air is transposed out of D into D sharp by opening these ducts, the key note of the original mode D must be used for the sharp seventh of that which is adopted, instead of C sharp, which, when fingered in the usual manner, is too flat; for the same reason it is also preferable in the scale of D sharp, to use the common C instead of B sharp for the major sixth major; and when an air is transposed out of middle C into C sharp, to use the common C sharp for the key note.

These, and some other defects, induce me to think that the following method of transposition by one pipe will be most eligible, as the fingering of the scale in any common key need not be altered.

Insert one end of a round pipe, three-tenths of an inch in diameter, and one inch long, into the inside of a German flute, so much nearer the holes for the fingers than the sound hole, that a line which encircles the flute, and passes through the middle of this last hole, may be seven-tenths of an inch from the centre of the interior orifice of the pipe, and all the sounds of the flute will be raised about half a tone when the ends of the pipe are open. Turn this pipe by the side of the flute, and let its exterior orifice be closed by a valve or key, which may be opened by the thumb of the left hand.

In the model with which I made the experiment, the pipe was inserted into the contrary side of the flute to that into which it should be inserted, when the instrument is intended for practice: it is delineated in this position by the two dotted lines in Fig. 6. I have not had an instrument constructed upon this plan, but I think it much preferable to the method proposed before. The flute may have all the common keys, and the fingering of no scale

will require any change after it is transposed a semitone higher. The B flat key, however, cannot be used in scales that are transposed; but there will not be much occasion for it at all, as the long key here recommended will supply its place, and be as easy to manage.

The pipe delineated by the two dotted lines in Fig. 6, was filled with water while each note of the flute was sounding, but the variation produced in one or two notes of the second octave by the ascent of the water was scarcely perceptible. Entertaining some doubt, however, that B and its octave were not perfectly in tune with each other, I compared them both with two corresponding sounds of an organ which were perfect octaves. The higher sound of the flute was a true octave to the sound of the lower toned organ-pipe, but the lower was rather too sharp to be exactly in tune with the sound of the higher pipe, unless the flute was carefully blown. After the transposing tube was removed, the relation between these two last sounds was little amended.

The octaves of A and B, rendered sharp by opening the end of the pipe, were as true as the octave sounds of German flutes are generally found to be when compared with other sounds which are perfectly in tune. Without making any further remarks on this method of transposition, I shall proceed to other objects.

Wind instruments of music, which have their notes regulated by several apertures, will generally produce two or more sounds, differing considerably in acuteness from one another, when all their holes are closed, or when any number of them are open, if the strength of the current of air employed in sounding the instrument be gradually increased: All the sounds in any series thus produced above the lowest of the series, are called harmonics: thus when C is the lowest sound of a German flute, the harmonics are C, G, c, e, g.

In the scales of the flute, the oboe, clarionet, and other instruments, a succession of the lowest sounds in diatonic order are produced by opening several holes, until the highest is only a tone or half a tone lower than the lowest harmonic of each instrument with all the holes closed. This harmonical sound is then taken into the scale, and another series is produced by opening the holes a second time, while the current of air is accelerated.

On the flute and the oboe these latter sounds, the aspiring offspring of those below, should be each an octave to its progenitor, but on the clarionet each should be a twelfth. The number of apertures that will be required to complete the lower series, depends upon the number of diatonic notes which compose the interval between the lowest sound of the instrument and its lowest harmonic: each note included between these must have an aperture to regulate its pitch. When the interval is an octave, six holes will suffice for this purpose; but when it is a twelfth or compound fifth ten must be employed.

The requisite pitch of each note is obtained by adjusting the sizes of the holes, and making them dilate considerably as they approach the inside of the instrument. We have before noticed the effect of opening a pipe which was all throughout of the same width, connected with the inside of a flute, and from what has been said, it is easy to understand why

why this shape in the holes is so universally adopted. As every aperture must regulate two sounds, it may justly be considered a desirable requisite, that the width of every wind instrument should be so proportioned, that the interval between each of those sounds produced with the least effort, and these propagated by increasing the force of the current, shall equal either an octave or a twelfth. Several adventitious circumstances, however, often destroy the harmonical relation which might otherwise result from the dimensions of the tube.

The harmonical part of the scale of the German-flute is nearly true, but the same part of the scale of those instruments, whose sounds are propagated by the vibrations of a piece of reed, are generally very imperfect. I shall only make a few observations respecting the clarionet, as I have made no experiments with any other instrument of this kind.

The pitch of the higher notes in the scale of the clarionet depends very much upon the strength of the reed, when the instrument is only blown, and the reed not pressed by the lips of the performer. A reed that is strong enough to perform the higher sounds in tune, unassisted by pressure, will soon fatigue the performer, and scarcely play the sounds of the lower scale: on the contrary, a reed that is slender enough to play the lowest succession of sounds full, will not play the scale of twelfths true, unless it be pressed strongly by the lips, and if any of the holes are widened so much as to produce the requisite pitch in the harmonical sound, then its progenitor will be made too high, compared with another sound which should be its octave: thus it happens, that adjusting the sizes of the holes to the sounds in one part of the scale, destroys their conformity in another part. It is however absolutely necessary, that the octaves should be true, and that the reed should be so slender as not to fatigue the performer; and therefore the higher sounds are generally made in tune with the lower, by pressing the reed—a method which is not agreeable, and requires much practice to regulate.

If every aperture of an instrument only regulated the pitch of one sound instead of two, the temperament of the scale might be easily adjusted, but from the limited number of holes that can be conveniently managed by the fingers, a series of diatonic notes could scarcely be carried through two octaves. An instrument upon this principle, I have frequently thought however, would serve very well for performing the base, or some part between the base and the treble, accordingly I have constructed one in the shape of Fig. 7, with a reed like a clarionet.

Fig. 8. is a section, which contains the proportionate length and breadth of the instrument upon a scale of one fifth of the real size. The inside of the tube A is five lines in breadth, and seven in depth. To this tube is fixed the piece which carries the reed, and two other pieces or branches B and C, which have the same dimensions as A. There is also a small pipe represented under the figures 1, 2, 3, 4, which opens into this tube. The instrument has nine apertures on its higher side marked 1, 1, 2, 3, 4, 5, 6, 7, 8, which are closed by the fingers; and two on its under side marked 9, 10. The ninth hole is closed by a valve or key, and the tenth by the thumb of the left hand. The end of the small pipe

at 5 opens on the under side, and is closed by the thumb of the right hand. The holes marked 11, 12, are always open, they are on opposite sides of the tube, and let out the air when all the other apertures are closed.

The lowest sound of the instrument is C, represented by the second space under the line marked F by the base cliff. The sounds D, E, F, G, A, B, C, D, E, F, are produced by opening the holes 10, 9, 8, 7, 6, 5, 4, 3, 2, 1, 1, and these are all the notes that can be conveniently obtained in this manner, unless there be a key for the foremost finger of the left hand to add G, which sound however, is as easily produced by closing all the holes except those marked 1, 1; after this, by opening the hole 8, while 1, 1, are open, the next note A will be added to the scale. Other notes still higher may be thus obtained, but they are not so easy to command. The small pipe under 1, 2, 3, 4, is made of a thin piece of brass; it is flat, and forms part of one side of the tube A; if it was round, it would be two lines in diameter. When any of the chromatic semitones between C and G are introduced, the end of the pipe is opened, and also if it is requisite to change the key of C for that of C sharp.

The holes marked 1, 1, are each in a different part of the tube, but as they are only separated by the thin division between the tubes A and C, they are as easily closed by the foremost finger of the left hand as one hole. When the aperture 1 in the tube A is only open, the sound G is too flat, but when the hole 1 in the branch C is also opened, it is a true twelfth compared with the lowest sound of the instrument, although the second sound, which is produced by strengthening the current of air when all the holes are closed, is only a compound fourth.

The branch B brings the hole 7 conveniently under the little finger of the right hand. The width of a part of this branch is contracted by the insertion of a thin plate in the inside, as is faintly represented in Fig. 7.

The hole 8 is closed by the little finger of the left hand: it is made wide, that the sound F may be true when the holes 9, 10, are either closed or open. The length of the reed, and the slanted surface of that part over which it is placed, may be determined by the lines drawn under Fig. 8. the higher line represents the flat side of the reed, the lower shews the inclined surface of the instrument.

This instrument is portable and easy to play; the intensity of its tone may be varied without any variation in its pitch; it requires a piece of reed which is very slender, and may be shaped in a few minutes. The scales of C and F major, and A and E minor will perform the best, but these may be transposed half a tone by opening the small pipe under 1, 2, 3, 4. The music may be written with the C cliff placed upon the lowest line but one of the staff.

The compound fourth produced by this instrument in place of the twelfth or compound fifth, suggested an alteration in the scale of the clarionet, namely, by a slender reed to substitute a scale of elevenths, or even tenths in place of the succession of twelfths.

Fig.

Fig. 9 represents a metallic clarionet, which was constructed for this purpose. It will perform the notes of a diatonic scale to the extent of two octaves and a fourth. The common holes for the fingers are marked 1, 2, 3, 4, 5, 6, 7: between 3 and 4 there is an additional hole for the little finger of the left hand, and about half an inch nearer the reed than the first, and on the opposite side of the tube there is an aperture for the thumb. The proportions of this figure are equal in length to one fourth of those of the instrument. The tube is five eighths of an inch in diameter beyond the figure 7; at 1 it is about half an inch. The lowest sound of the instrument with all the holes closed, is G represented by the space immediately above the line, which causes the base cliff. When the instrument is blown easily, and the holes 7, 6, 5, 4, *l f b*, 3, 2, 1, *t b*, are opened one after another, the following succession of notes will be produced, A, B, C, D, E, F, G, *a B*: when the hole 5 is opened for C, 6 should be closed. For B flat, close the hole, 1 while the thumb hole is open. For C the compound fourth of the lowest sound G, close all the holes except the first and the thumb hole, or either of them. While blowing a little stronger close these, and open the holes in the same order as for A, B, C, D, E, F, G, A, B, and the following succession of sounds will follow, D, E, F, G, A, B flat, B, C. If the highest C be too flat, open the thumb hole and close the second. Another sound still higher may be added, by pressing a key with the foremost finger of the left hand. The compass of the scale will then be equal to two octaves and a fifth. A small pipe might be introduced into the side of the instrument opposite the seventh hole, and its exterior end closed by the thumb of the right hand; when it is opened the lowest sound G, and C its compound fourth, will be raised half a tone. It would also be advantageous to have a joint near the wider end of this clarionet to vary the pitch of middle C, in the modes of C and F; when this note is raised a little, its octave below may be made true by opening the sixth hole. An instrument of this kind may be made of wood. It seems well adapted for performing the parts generally assigned to the *first* clarionet. All the sounds of its scale are nearly of an equal strength. The fingering of its scale is easy, as none of the common notes are introduced by opening apertures which are closed by inanimate valves or keys. The reed is slender, and requires little pressure from the lips in performing the higher sounds of the scale. Keys are not necessary in the diatonic scales of C and F; but for introducing F sharp in the lower series of notes, and E flat in the higher, two keys will be of advantage. A bass instrument as long again as this clarionet, with the same kind of a reed, and with the holes for the fingers disposed in the same order, may be made in a serpentine form like the part 1, 2, 3, 4, 5, 6, in Fig. 2, but if it be intended to perform an extensive scale, all dilated parts should be avoided, or the sounds in the higher series will be too flat when produced by a slender reed.

I shall finish this long letter with an explanation of the tenth figure. When the external air is excluded from the side aperture of an organ pipe, and the sound is carried with the current which produces it, through another pipe of the same dimensions as that which determines

determines the pitch, the sound is considerably weakened, and its pitch is raised. Fig. 10, represents the section of the instrument with which I made the experiment. It consists of two pipes A and B. A square pipe was constructed of metal, with an aperture in one side like those pipes of an organ in the flute stop. When its end was open its lowest sound was in unison with G above the fifth line of the treble staff.

A thin piece of metal was now turned over three sides of the piece of wood upon which the pipe A had been turned, and its edges were soldered to two corners of that pipe. Two tubes being thus constructed, the aperture for producing the sound was in the partition between them. When that end of the tube B which is nearest the side aperture was closed, and the instrument was blown, its tone was considerably changed, and a full note higher.

An instrument of this kind may be used when any of the gages are employed in the production of sound.

The harmonical sounds of this pipe are A, octave to its lowest sound; E, twelfth; A, double octave or fifteenth; and C sharp, seventeenth major. I consider the change in its original tone and pitch to be owing to the diminution of pressure upon the side aperture. Flame produces nearly similar effects. Bring the flame of a candle very near the side aperture of a small organ pipe, and its sound will rise considerably.

I think a musical instrument might be formed upon a plan nearly similar to that of Fig. 10. After the proper length of the pipes A and B is determined, six holes may be made into each, so near the partition, that all the twelve may be closed by six fingers.

Our small wind instruments have many imperfections, but are the objects of so little direct importance to society, that we do not expect much celerity in their improvement.

I am, SIR,

Your humble Servant,

WILLIAM CLOSE.

Dalton, July 24, 1801.

IV.

On the Production of artificial Cold by Means of Muriate of Lime. By Mr. RICHARD WALKER. (Philos. Transf. 1801.)

THE subject of the means of producing artificial cold, or the constitution of frigorific mixtures, I had considered as exhausted, in the Papers I have already had the honour to lay before this Society: so far as relates to that part of the subject which consists in generating artificial cold *without the use of ice*, it still remains so with me, having nothing new to offer.

A con-

A considerable acquisition however having been made, since my last Paper "On the best Methods of producing artificial Cold,"* by the discovery that a neutral salt, but little known, or attended to, by chemists before, produced extraordinary effects of this kind with ice, it could not fail of attracting my attention.

Since the time I first became acquainted with this circumstance, I have, as opportunity offered, been engaged in making a variety of experiments with this salt, which I flatter myself, if the Society have not already received a communication on the subject, may not prove unacceptable.

Before I relate my own experiments, it may be proper to premise a short account of those of Mr. Lowitz, the author of the discovery.

The result of Mr. Lowitz's experiments are, in *his* Memoir,† given according to the scale of Reaumur; but, in *this*, are throughout reduced to that of Fahrenheit.

Mr. Lowitz, professor of chemistry in Petersburg, having found, by an experiment made in the winter of 1792, that caustic vegetable alkali, in a solid state, produced a degree of cold far exceeding any other substance before mixed with snow, *viz.* 83 degrees, determined to prosecute the subject; and, upon reflection, considering that the deliquescent salts were likely to be fittest for his purpose, fixed chiefly upon the class of muriatic salts, or those which have their base neutralized by the muriatic acid. The result of his experiments was the discovery, that crystallized muriate of lime sunk the thermometer 82 degrees; and that the other neutral salts of this class, though much inferior to that salt, exhibited nevertheless remarkable powers of the same kind.‡

Professor Lowitz, in the Memoir alluded to, observes that he has repeated my experiments with chemical salts and *snow*, but could not produce a degree of cold below $+ 2^{\circ}$. Here is evidently some mistake; for it is sufficiently known, that the novelty of my experiments depends on the production of cold, *without the use of ice* in any form. §.

Pr. Lowitz having found by experiment that, at the temperature of $+ 27^{\circ}$, *four* parts of muriate of lime to *three* of snow produced a temperature of $- 55^{\circ}$, and that an increase of the salt, even in the proportion of two to one, did not diminish the effect, determined the best and *surest* proportions to be, three parts of the muriate of lime to two of snow.

But, since we shall find hereafter the temperature of $+ 32^{\circ}$ to be a more convenient term of comparison, we may fairly state the fact thus; that muriate of lime three parts, and snow two parts, mixed at the temperature of $+ 32^{\circ}$, will give $- 50^{\circ}$.

The snow, to produce the greatest effect, he says, should be fresh-fallen, dry, and light or uncompressed; and the salt perfectly dry, and reduced to very fine powder.

* Phil. Trans. for 1795, p. 270.

† See a translation from Crell's Chemical Annals for 1796, by Mons. Van Mons, Vol. XXII. p. 297, of the *Annales de Chimie*.

‡ Professor Lowitz no sooner discovered the great efficacy of the muriate of lime for this purpose, than he gladly rejected the caustic vegetable alkali, on account of its burning quality; the difference being *one* degree only.

§ See the table of frigorific mixtures. Phil. Trans. for 1795, p. 279.

Pr. Lowitz's method is, to add at once the salt to the snow; the latter being previously put into a convenient vessel. As the salt produces the greatest effect whilst it retains the greatest quantity of water of crystallization, he prepares it during a freezing atmosphere, pounds it, sifts it, and keeps it in close bottles, in a cold place. With a mixture of this kind, Pr. Lowitz froze, in one experiment, 35 pounds of quicksilver.

Pr. Lowitz observes, that with the above precautions and management, it is impossible to fail in the design of freezing quicksilver with it.

Pr. Lowitz found likewise, that the muriate of lime, prepared as above, produced 38 degrees of cold by solution in water; that is, by adding 3 parts of this salt, in powder, to 2 parts of water, each at the temperature of $+36^{\circ}\frac{1}{2}$, the thermometer sunk to $-1^{\circ}\frac{1}{2}$.

Pr. Lowitz adds, that the muriate of lime which has been used for making frigorific mixtures, may be procured again repeatedly, as fit as at first for the same purpose, by evaporation and crystallization.*

Having given an account of Pr. Lowitz's experiments, I shall now briefly relate the result of a series of experiments made by myself, which occurred to me in consequence of Pr. Lowitz's discovery.

My first object was, to repeat the foregoing experiment under similar circumstances; but the thermometer (the temperature of the air and materials being $+32^{\circ}$) sunk, in this instance, no less than -48° .

The liquefaction, in the above instance, was remarkably sudden; and the full effect was produced, as it were, in an instant.

Secondly, with a view either of producing a very extraordinary degree of cold, or ascertaining the point at which this salt ceases to produce further cold, I mixed the same materials, previously cooled by art to 40° below 0, when the thermometer sunk to 63° below 0.†

Thirdly, some of the same salt, in a crystallized state, was set out to deliquesce in the open air: this liquor mixed with snow, each at the temperature of $+32^{\circ}$, gave a cold of -20° .

Hence it appears, that 52 degrees of heat were absorbed, or rendered latent, by the liquefaction of the snow, and 28 by that of the salt, in the first experiment; that is, in the whole, 80 degrees.

The muriate of lime used in the above experiments was prepared thus: muriatic acid 1 part, and distilled water 3 parts, were thoroughly mixed; this liquor was then perfectly saturated with whiting, and, when clear, poured off. The mixture was afterwards evaporated, till it crystallized in air at $+32^{\circ}$, (which happened when the liquor had been evaporated to the consistence of a thin syrup,) and then reduced to fine powder.

* The muriate of lime made use of by Professor Lowitz, in these experiments, was obtained from the residue after the distillation of caustic ammonia.

† In this instance, as I afterwards found, the proportion of snow, owing in part to a considerable portion of the muriate of lime remaining frozen at the bottom of the vessel, and not mixing, was much too great.

From

From the preceding account it is apparent, that Pr. Lowitz has discovered a single frigorific mixture, by which quicksilver may be frozen whenever the temperature of the materials at mixing is no colder than $+32^{\circ}$; whereas, the *nitrous acid* with snow, which has hitherto been considered as the most powerful of frigorific mixtures, requires a temperature of $+7^{\circ}$, to produce the same effect.

At the same time, however, it should be observed, that an experiment with this salt, prepared as above, can be made only *during a freezing atmosphere*; the salt itself thus prepared, becoming, as may be inferred from the above, and as I have myself experienced, unfit for use by a warmer temperature.

Reflecting upon my former experiments, I determined to try the effect of this salt, reduced to such a strength, by evaporation, as to endure being kept, in a solid state, throughout the year. For this purpose, I found it necessary that the liquor, prepared as before, should be evaporated to the consistence of a thickish syrup, before it is set by to crystallize; when the produce will be a semi-transparent, uniform, crystalline mass, which affords, on pulverizing, a white pearl-coloured powder, which should be preserved for use in a bottle closed with a ground stopper.

Some of the powder above mentioned, (which had been previously subjected to a temperature of $+70^{\circ}$, without melting,) I mixed with snow, each at the temperature of $+32^{\circ}$; when the thermometer, to my perfect satisfaction, sunk to -40° ; and, at another time, when the air was $+20^{\circ}$, I froze quicksilver perfectly solid, by a mixture of the same materials.

It appeared reasonable to expect that the power of this salt for producing cold, might be assisted by the combination with, or addition of, other salts, as has been found to be the case in other instances, to a considerable degree; and, conceiving from my former experiments, that the muriate of soda and nitrate of ammonia promised the greatest success in this way, these alone producing with snow a cold of -25° , I was naturally led to combine these with the muriate of lime; but I obtained no advantage by this, or by any other means, worth noticing.*

In the course of last winter, and the summer preceding, I repeated my former experiments with more accuracy; and likewise made some additional ones, on the power of muriate of lime for producing cold with ice, the result of which I shall here briefly state.

In order to reduce the experiments made with the muriate of lime to a greater certainty, I carefully obtained the respective specific gravities to which this salt should be reduced by

* M. Van Mons tried the effect of the caustic soda (which alone produces a cold of -15° with snow) combined with the muriate of lime, and found the effect increased very considerably. By this means, he says, "In my new experiments on the effects of artificial cold, I have carried it to -53° , old division"; (viz. 87° of Fahrenheit.) He does not mention the temperature at which the materials were mixed; to produce this effect.---M. Van Mons effects a chemical union of these two salts thus: he slakes quicklime with a solution of muriate of soda; this mixture, when become cold, he filters, and afterwards evaporates, until when cold it will become solid. *Annales de Chimie*, tom. XXIX.

evaporation, before it be set by to cool, in order to become solid, in either instance: thus, when the muriate of lime is to be of that strength which is to be prepared and used at the temperature of $+ 32^{\circ}$, the specific gravity of the liquor should be, at the temperature of $+ 80^{\circ}$, 1,450; and when of that strength to be kept and used at the ordinary temperature of the air at any time, the specific gravity of the liquor should be 1,490, at 80° of heat.*

The liquor, when sufficiently evaporated, should be set by to crystallize; and the crystallized mass, as soon as cold, should be reduced to very fine powder, in a glass or stone mortar. The muriate of lime, in its solid state, being a hard brittle substance, it is necessary commonly to immerse the vessel containing it in water sufficiently warm to loosen the mass, in order to remove it out of the vessel, to pound it.

When the muriate of lime is intended to be preserved for future use, the powder should be put directly into a bottle, and closely stopped from the air; for this salt is extremely deliquescent, and hence, a dry state of the atmosphere should be chosen for preparing it.

These experiments I shall divide into two series; the first of which consists of those made with the muriate of lime prepared so as to be used in *winter* only, that is, of the strength of 1,450.

The second series consists of those made with the same salt prepared so as to be kept for use at any time, the strength of which is 1,490.

SERIES I.

Exp. 1st. Muriate of lime	-	3,	Snow 2,	-	-	at $+ 32^{\circ}$	-	-	gave	-	$50^{\circ} +$
2d. ————	-	2,	——— 1,	-	-	——— 0	-	-	———	-	66
3d. ————	-	3,	——— 1,	-	-	——— 40	-	-	———	-	73
4th. Diluted vitriolic acid	10, †	——— 8,	-	-	-	——— 68	-	-	———	-	91 §

SERIES

* Muriate of lime evaporated to the strength of 1,400, gives (if cooled slowly in a cold air, viz. $+ 20^{\circ}$) perfect crystals; this is the fittest state of the salt for producing cold.

† This experiment being repeated, using ice-powder, (instead of snow,) gave $- 51^{\circ}$; — that is, ice ground to very fine powder with the instrument described in Phil. Trans. for 1795, page 288. (Philos. J. I. 498.)

‡ Concentrated vitriolic acid 8 parts, water 4 parts, and rectified spirit of wine 1 part, mixed and cooled previously to the temperature of the air.

§ This experiment was made on the 10th of March last, and conducted by a series of three mixtures, thus. The materials for the *second* mixture, consisting of muriate of lime and snow, separated from each other by an intervening stratum of fine sand, were cooled in a large vessel, having a partition in the middle forming it into two compartments, to near 40° below 0, by means of a *prior* mixture of the same materials. The materials for the *second* mixture, cooled as above mentioned, were then mixed in each compartment of this double vessel, and let through an aperture at the bottom of each, (closed till then by a temporary partition,) into such an apparatus as that represented in the drawing, which contained the materials for the *third* or last mixture, consisting of the diluted vitriolic acid and snow, which had already been separately cooled in their respective vessels, to near 40° below 0.

The

SERIES II.

Exp. 1st.	Muriate of lime	- -	5,	Ice-powder	- -	4, at	+ 32°, gave	- 41 *
2d.	_____	_____	4,	_____	- -	3, - -	20 - -	- 48
3d.	_____	_____	4,	_____	- -	3, - -	10 - -	- 54
4th.	_____	_____	3,	_____	- -	2 - -	15 - -	- 68

In the first experiment, the materials were mixed at the temperature of the air.

In the second, they were previously cooled, by a mixture of muriate of ammonia, nitrate of pot-ash and water; temperature of the air 52°; the salts which formed the preparatory mixture being recovered for use again, by evaporation.

In the third, they were cooled previously, by a mixture of muriate of ammonia, nitrate of pot-ash, fulphate of soda, and water; temperature of the air 50°. And,

In the fourth, by a mixture of phosphate of soda, nitrate of ammonia, and diluted nitrous acid; temperature of the air 49°. †

Having concluded my experiments with the muriate of lime and ice, I proceeded to try the effects of this salt, prepared so as to retain its solid state during summer, *viz.* of the strength 1,490, by solution in water; and found that a mixture of this kind, produced twenty-nine degrees of cold; for, by adding 5 parts of the muriate of lime, in fine powder, to 4 parts of water, each, at the temperature of + 50°, I obtained a cold of + 21: this effect was not improved by the addition of other salts.

These experiments were made in such an apparatus as I am going to describe, which may at first appear new, but, upon examination, will be found to be only a different modification of the vessels represented in Fig. 3 and 4, Tab. XXIII of Phil. Transf. for 1795; (Philos. J. I. Plate XX.) and appears to me to have all the advantages that can be obtained in an experiment of this nature.

The thermometer represented in the drawing was then placed in the tube of the upper vessel; and, when the snow was cooled to the utmost, *viz.* to near 70 below 0, the cooled snow was forced through into the cup containing the acid, the vessels were separated, and the snow and liquor thoroughly mixed by means of the thermometer contained in its glass tube; the thermometer was then withdrawn from the tube, and stirred about in this last or third mixture, which, in ten seconds of time, indicated a cold of 91½ below 0; twenty seconds more elapsed before the thermometer began to rise. The mouth of the cup in the vessel B was, in this instance, closed with waxed paper, in order that I might invert the vessel occasionally, to renew the mixture in it; and the cup itself was coated within-side with wax, in order to defend it from the action of the acid.

* The same experiment made at + 32°, with snow, (instead of ground ice,) gave - 40°.

† The composition and application of these frigorific mixtures are given in Phil. Transf. for 1795.

(To be concluded in our next.)

V.

*Directions for extracting Gum from the Lichen, or Tree Moss, &c. By the EARL OF DUNDONALD.**

IT does not appear from such trials as Lord Dundonald has hitherto made, that there is any very great difference in the produce of gum from the lichen collected from different trees or shrubs: all of them answer equally well for yielding a gum fit for calico printing. The lichen is most abundant on the trees which grow in a poor, stiff clay soil, particularly if situated at some considerable height above sea-level. It should be pulled in dry weather, otherwise it is apt to break in the pulling; besides, in this case, requiring to be dried, before it can with safety be laid up in the storehouse, where, if put in dry, it may be kept for years. Should a sufficient quantity of it not be found in this country, it may be had in almost unlimited abundance in Sweden, Norway, and in the northern parts of America, where it grows to the length of from a foot to eighteen inches, depressing the branches of the tree by its weight. There is, however, every reason to believe that a sufficient quantity is to be had in this country. According to information from Dr. Brown, lecturer on botany, it takes three or four years in coming to maturity or its full size, so that a crop from the same tree may be had every fourth year.

The lichen does not consist entirely of a gummy matter. There is the outer skin or cuticle, below that a green resinous matter: the remainder of the plant consists of partly gum, partly a matter somewhat analogous to animal substances, and a small proportion of fibrous matter, which cannot be dissolved by boiling, or the action of alkaline salts.

The first process in preparing gum from the lichen is to free it of the outer skin of the plant and the resinous matter. This is done by scalding the lichen two or three times with boiling water, allowing it to remain so long in the water, as by absorbing it to swell; in doing this the skin cracks and comes off along with the greatest part of the resinous matter. Or it may be freed from them by gently boiling the lichen for about fifteen or twenty minutes, then washing it in cold water, laying it afterwards upon a stone or brick floor, where it should lie for ten or twelve hours, or perhaps more. The reason for this is, that the exposure for that time to air greatly facilitates the subsequent extraction of the gum.

The scalded lichen is then to be put into a copper boiler, with a due proportion of water, say three Scotch pints, or two wine gallons, to every pound of lichen, and boiled

* Early in the present year this Nobleman convened the calico printers of Scotland; and communicated to them this important discovery, at a meeting at Glasgow, where several resolutions were made for the purpose of securing its advantages to the country, with a proper reward to the inventor. The present paper was printed at Glasgow, in six pages duodecimo, 1801. The statement which follows was also drawn up by his Lordship, by whom I was favoured with both,

during

during four or five hours, adding about half an ounce or three quarters of an ounce of soda or pearl ashes for every pound of lichen; or, instead of these salts, about half an English pint of volatile alkali. The boiling should be continued until the liquor acquires a considerable degree of gummy consistence. It is then to be taken out of the boiler, allowed to drain or drip through a wire or hair cloth, or searse. The residuum to be put into a hair cloth bag or bags, and to be squeezed in a press similar to that which is used by the melters or rinders of tallow.

The first boiling does not extract the whole of the gum. The lichen should be boiled a second and even a third time, repeating the process as above described, diminishing at each process the quantity of water and the quantity of alkali, which a little experience will soon point out. When three boilings are employed, the gummy extract of the last boiling should be kept for the first boiling of a fresh batch of lichen. The extract proceeding from the first and second boilings should be mixed together, and evaporated to the consistence necessary for block or press printing. The evaporating vessels should be of tin or thin lead placed over a range of stoves and moderately heated by fire or the steam of water. It has been neglected to state, that before evaporating the gummy extract to the consistence necessary, it should be kept ten or twelve hours, so as to allow the sediment or dregs to subside. The clear liquor may either be drawn off by a syphon, or the dregs may be drawn off by a cock at the bottom of the wooden vessel; the bottom of which should be made sloping, higher at the back than the fore part, in order that the dregs may run more completely off. The proportion of gummy matter remaining in the dregs may be got off by mixing them with a due proportion of boiling water, allowing the liquor to clear, and proceeding as above directed, employing this weak solution for boiling the next batch of lichen.

The residuum of the lichen, after the third boiling, consists of a matter somewhat analogous to that of animal matter, together with a proportion of the fibrous matter of the plant. From the animal matter a kind of soap may be made. The process is as follows:

Let a small proportion of pounded rosin be dissolved by boiling in a solution of alkaline salts. When the rosin is dissolved, put in a certain quantity of the residuum of the lichen, and continue the boiling until this last article is pretty well dissolved. Then add about a pound or a pound and a half of white soap shaved down or cut into thin slices to every ten pound weight of the residuum of the lichen. Continue the boiling until the white soap is fully dissolved. Then pass the soapy solution through a hair sieve or hair bag. Let the soap pan then be cleaned out and the soapy solution returned to the pan, to be boiled to the consistence necessary. A kind of soap very similar to this may be made from sea weed, as well as from several other articles. Of this and of other matters, highly important to calico printers, a more full and detailed account will be given in a future publication. When gum from the lichen is to be employed for making ink, manufacturing and staining paper, and

and for stiffening silks, crapes and gauzes, it should be extracted from the lichen without employing any alkaline salts, continuing the boiling or digestion longer, and with a moderate degree of heat, in which case the gummy extract will be nearly colourless.

When volatile alkali is used, the boiler should be of iron, as volatile alkali acts on copper.

Statement of the Benefits and Advantages to ensue to Individuals and to the United Kingdom of Great Britain and Ireland, from Lord Dundonald's Discovery of extracting Gum from the Lichen, or Tree Moss, as a Substitute for Gum Senegal, and other Foreign Gums.

Gum Senegal, or other foreign gums, have hitherto been indispensably requisite for fine goods, and those of light shades of colour.

No proper substitute had been found to answer for this part of calico printing, until Lord Dundonald discovered the preparing gum from the lichen.

Gum Senegal, and foreign gums, are at times scarce, and difficult to be procured.

The settlement of Senagambia belongs to the French. They have a monopoly of gum Senegal. The price since the war has risen from 150l. to 400l. per ton.

Gum from the lichen may, all charges included, be prepared at one-fourteenth part of the present price of gum Senegal, and at one-sixth of the peace-time price.

The United Kingdoms may now be said to be independent of foreign states for the gum requisite for their manufactures, such as calico printing, preparation of ink, staining and manufacture of paper, dressing and stiffening silks, &c. &c. And, in consequence of Lord Dundonald's discovery, large sums of money formerly sent abroad for the purchase of gum, will, in future, be saved.

Starch and wheaten flour have hitherto been used as cheap substitutes for gum, in printing coarse goods, or to fix on other printed goods, the mordants, and some of the dyeing materials, for deep shades of colour. But there is now reason to expect, as gum from the lichen will cost less than those articles, particularly starch, it will be used in their stead, and so prove the means of saving, or economizing a considerable proportion of wheat and wheaten flour, at present consumed in calico printing.

The collecting the lichen from the forest and fruit trees and hedges, will give employment to a number of poor people, principally women and children.

A cheap supply of gum from the lichen, may perhaps, in several manufactures, supersede the use of the higher priced article of glue or size, which often has a very disagreeable smell.

Gum from the lichen, may perhaps, be used as a dressing in weaving, particularly fine goods, either by itself, or when made into a kind of soap, according to directions given in a separate publication.

Lastly,

Lastly, It is probable that all the benefits to ensue from the discovery of procuring gum from the lichen may not yet have occurred, it being well known, that there is no art or discovery that is not the parent or sister of some other.

VI.

On the Nature of Galvanism, Electricity, Heat, the Composition of Water, and the Theory of Chemistry. By a Correspondent.

TO MR. NICHOLSON.

SIR,

THE more I consider the objections which I started respecting the explanation of the phenomena on the application of the influence from the pile of Volta to water by means of wires, the more am I convinced that the system of chemistry, according to M. Lavoisier, is not applicable to the appearances which are continually presenting themselves. However that system may be sanctioned and supported by the consent of eminent philosophers, yet I trust that in every discussion of subjects of natural philosophy, the laudable desire of acquiring truth will outweigh any presumption which may appear in an opposition of such favoured doctrines.

The communication from Dr. Babington, which appeared in your late Journal (IV. 511), was decisive. It clearly proves, what I had ventured to suggest, that water is not composed of the bases of oxygen and hydrogen air. If the two gases separated from water by the pile were collected, and the hydrogen air burnt in the oxygen air, would not in that instance the two kinds of electricity be converted into ordinary culinary fire? It appears that on burning those airs together, the water, when collected, has lost none of its weight; if so, does not the fire produced originate from the positive and negative electricities? If water is not decomposed by the pile, whence comes the hydrogen? Hydrogen not being a component part of water, its name surely ought to be changed. If the original weight and quantity of water be again collected after the combustion of those airs, which have been supposed to be its constituent principles, by what process of reasoning can we assent to the bases of those airs being peculiar elements or radicals. If the matter of heat were a simple body, and if the gaseous state of these radicals depended on the greater capacity they have for caloric, whence comes the difference observed between them when they are becoming fixed; and why does one kind of electricity invariably produce the same air? The following is the manner in which the phenomena of the solid, liquid, and gaseous states of bodies are described according to the theory of M. Lavoisier.

All bodies, from their combination with caloric, may exist in three different states, which result from:

I. The

1. The power of attraction by which bodies cohere.
2. The quantity of caloric with which the substance is combined.
3. The greater or less pressure of the atmosphere.

The means by which a solid body may be converted into a liquid are therefore

1. A combination with a new and sufficient quantity of caloric.
2. An union with another body already liquid.

A liquid is changed into the state of gas

1. By being combined with a quantity of caloric sufficient to equipoise the pressure of the atmosphere.
2. If the pressure of the atmosphere be either sufficiently diminished or totally removed.

According to this system, therefore, there exists no difference between gaseous bodies but from the difference of their respective bases; the caloric is common to all. Why, therefore, does oxygen air, when united during combustion with charcoal, give out fire with flame, whilst other gases, even when instantly changed into a solid, give out only heat? Or why is one portion of water from the pile wholly converted into oxygen gas? The minus side certainly cannot form a gas by abstracting fire, even though it should be in the state of electricity.

It appears, therefore, that the theory of M. Lavoisier is defective, in as far as it proves neither the decomposition of water, nor the nature of permanently elastic fluids. I could bring forward many experiments in a new form to prove the fallacy of the system of M. Lavoisier, but we have already sufficient facts before us to demonstrate upon what a slight foundation it rests.

The explanation of most of the phenomena of chemistry is involved in this discussion. The nature of the atmosphere and of vapours, combustion and all the appearances of light, heat and electricity, chemical solution, decomposition, &c. in short, the whole system of M. Lavoisier totters, since we know that water is not composed of the bases of oxygen and hydrogen air.

As I mean this only as a notice, I shall not insist further at present on the subject. As I mean to pursue it systematically and by experiment, I hope I shall not intrude too much upon you by occasionally submitting to you the results of my investigations.

I have the honour to be

Your obedient humble servant,

AN EXPERIMENTALIST.

March 7, 1801.

P. S. I find that heat is easily decomposed, and that the Galvanic influence may be detached from a very great variety of substances. *

* I am sorry to be under the necessity of apologizing to this Correspondent for the late appearance of his letter, which was occasioned by its having been placed by mistake among some other papers of a different nature. - N.

VII.

*Notice respecting the new Method of Bleaching, and its Application to the Washing of Linen Garments. By CITIZEN CHAPTAL.**

TWENTY months ago I communicated to the Institute a process, no less simple than economical, by means of which cotton goods could be bleached in a very short time.

This process was speedily rendered public by the journals, and the English availed themselves of it to such advantage, that, a short time afterwards, it was adopted and brought to perfection in the capital manufactory of linen which exists in Ireland.†

The success of this process, in the hands of a rival nation, has awakened the exertions, and excited the interest, of some French manufacturers. Citizen Bawens, proprietor of a manufactory of cotton-yarn and piece-goods, at La Barriere des bons Hommes, applied to me for information respecting this process. I gave him the dimensions of the apparatus, and the construction was intrusted to Citizen Bourlier, a distinguished chemist, and proprietor of the sal ammoniac work at Gros-Caillou. The first experiment, which was made in the works of Bawens, was performed upon 2000 metres of cotton in the piece. The result was so advantageous in every respect, that Citizens Bawens and Bourlier associated together to form similar establishments in various parts of the Republic. Citizen Bourlier is at this instant in Belgia, where he has established works of this kind to such an extent, as to be able to bleach a large proportion of the goods which are manufactured in that country.

I must observe that the process which I have described has been singularly improved by Citizen Bourlier. He has contrived easy methods of turning the goods in the apparatus, and presenting all their parts to the vapour. He has found that linen requires in fact only a weak ley; but that it is necessary to alternate the action of the ley with that of the atmospheric air, in order to bleach completely. He has succeeded in giving a degree of whiteness in two or three days to the coarsest goods which the usual process of the bleachery could not communicate but in a much longer time, and with more expence.

I have the honour to present to the class samples of dimities and cloths bleached by Citizen Bourlier. It will be seen that the colour is equal to any thing which has hitherto appeared in the market.

I shall not enlarge more fully on the advantages which commerce must derive from this method, but shall confine myself to observe that the first experiments, which are always the most expensive, did not carry the price of bleaching to more than half the ordinary price.

* Annales de Chimie, XXXVIII. 291:

† The details of this method may be seen in a work of Citizen D'Orelly, entitled, "Essai sur le Blanchiment;" sold by Deterville, Rue du Battoir, No. 6, at Paris.

I have no doubt but that linen garments may be bleached to advantage by the same process; but as it was necessary to bring these notions to the test of experiment, I invited Citizen Bawens to allow me to make the experiment on a large scale with his apparatus. Accordingly, on the 27th Pluvisoie, in the year IX. I had 200 pair of sheets from the hospital of the *Hotel Dieu* at Paris, chosen among those that were most soiled, and taken to the manufactory of Citizen Bawens.

Three experiments were made upon these sheets.

Experiment 1. One hundred and thirty sheets were impregnated with a caustic alkaline ley, containing one hundredth part of soda. They were kept for six hours in the engine of steam; after which they were impregnated again, in order to be placed again six hours in the same machine.

The same process was repeated a third time; after which they were carefully rinsed, and no spot of wine, grease, blood, or animal ejection, was to be seen. One quarter of a pound of soap was used in rinsing these cloths.

All the assistants were convinced that the ordinary processes would not have given either so perfect a white, nor so agreeable a lixivial smell.

The texture was in no respect altered.

Experiment 2. The alkaline ley contained only six parts of soda, but five pounds of soap were also added. The cloths were treated in the same manner, and the results appeared more advantageous. They were more easily washed out.

Experiment 3. To the bath of the second experiment a sufficient quantity of new ley was added. One hundred and forty sheets were treated like the preceding, and the result was the same.

It must be observed that the water of the Seine, in which these sheets were rinsed, was at that time very yellow.

This experiment appeared to me to offer several results worthy the attention of the Institute.

In the first place the process is economical. Two hundred pair of sheets, which were bleached by three successive operations, demanded an expenditure, according to the account furnished by Citizen Bawens, which is in the proportion of 7 to 10, compared with that commonly made in the hospitals. This expence may be reduced to less than one-third, if a suitable place and apparatus were appropriated to this use.

2. Two days at most are required to complete the operation. This oeconomy of time is incalculably beneficial.

3. The linen is neither changed nor torn, as it passes through the hands only once, and it is of no use to beat it.

4. The extreme heat to which the linen is exposed in the apparatus, causes its texture to be penetrated by the alkaline fluid to such a degree, that the substances with which it is impregnated cannot be masked from its action; so that the putrid exhalations, and other substances attached to its texture, are necessarily destroyed or changed in their nature.

This

This effect must be more particularly seen with regard to its value, by physicians, who are aware with what facility the seeds of various disorders are perpetuated in hospitals, and how insufficient the greater number of processes used in washing linen have proved to destroy them.

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

Notice relative to the Gadolinite. By CIT. HAUY.

THIS substance was discovered at Ytterby, in Sweden. In the year 1794, M. Gadolin announced the existence of a new earth; and his assertion has since been confirmed by M. Ekeberg's analysis of that substance, to which he has given the name of Gadolinite, in honour of the discoverer, and he has denominated the new earth Yttria, a name derived from the country where the mineral that contains it was discovered.

From the description published in the *Journal de Physique* for Fructidor, 8th year, it appears that the Gadolinite has a tolerably perfect black colour; it breaks in an imperfect conchoid form; it is brilliant, and its polish is vitreous; its specific gravity is also considerable.

To these external characters may be added others of a physical and chemical nature, already in part known, by which the Gadolinite is clearly distinguished from some other minerals to which it resembles in its aspect, particularly the vitreous lava denominated lapis obsidianus. Its specific gravity, which Cit. Haüy has found to be 4,0497, is greater than that of the above-mentioned lava, nearly in the proportion of 5 to 3; but it is less than that of black sulphurated uranium, called pech-blende, in the proportion of 2 to 3. The Gadolinite likewise, when reduced to powder and put into nitric acid, diluted with water, is discoloured on heating the acid, and converted into a kind of thick jelly, of a yellowish grey colour. According to the observations of Cit. Lelievre, the Gadolinite, when exposed to the flame of the blow-pipe, decrepitates, and emits particles apparently inflamed: but if care has been taken to ignite it by the flame of a candle, it does not decrepitate: it acquires a red tinge, intermixed with white; cracks, and does not melt, unless the fragment be very small. The Gadolinite acts very sensibly upon the magnetic needle, but Cit. Haüy has not discovered that it has poles.

M. Ekeberg obtained from the Gadolinite 47.5 parts of Yttria, 25 of silica, 18 of iron, and 4.5 of alumina: the loss was 5. Cit. Vauquelin, on repeating this analysis, found 35 parts of Yttria, 25.5 of silica, 25 of iron, 2 of oxide of manganese, 2 of lime, with a loss of 10.5, the cause of which he investigated, and attributes it principally to the water contained in the Gadolinite, and to a small quantity of carbonic acid. He observed that the new earth has some analogy to glucine. Like that substance it forms saccharine solutions

H h 2

with

with acids, but their taste is rather more acid, and more nearly resembles that of solutions of lead. The Yttria likewise differs from glucine, because it is not soluble (like the latter) in caustic alkalies; and because the salt which it forms with the sulphuric acid, instead of being soluble, as it is when glucine constitutes the base, is on the contrary scarcely at all soluble; and, lastly, because it is precipitated from its solutions in the acids by the prussiate of pot-ash, which does not take place with glucine.

The pieces of Gadolinite treated by Citizens Vauquelin and Haüy, for the purpose of ascertaining the nature and characters of that substance, were presented to them by Messrs. Abildgaard, Manthey, and Neergaard.

Soc. Philom. No. 44, An. 9.

Note relative to the new Earth denominated Yttria. By M. KLAPROTH.

THIS earth, which contains Gadolinite, not only possesses the common property of glucine, of forming salts of a sweet taste, but it also readily dissolves in a solution of carbonate of ammonia. It nevertheless has peculiar characters, which, distinguishing it from the earths, renders it in some degree similar to the metallic oxides. With the sulphuric and acetic acids, it forms crystallized salts, of a pale rose colour: it is precipitated from its combinations by tannin and by Prussian alkali.

The proportion of Yttria in the Gadolinite, stated by Ekeberg, is not very exact. The cause of this may probably be attributed to the method he employed to separate the Yttria from ferruginous substances, which induced him erroneously to imagine that the sulphate of Yttria is not decomposed by fire. This decomposition, however, does take place, but not so readily as that of the sulphate of iron. M. Klaproth has ascertained by his analyses that Gadolinite is composed of 21, 15. parts of silica, 18. of oxide of iron, 59, 75. of Yttria, and 0, 50. of alumine. This small quantity of aluminous earth is probably merely accidental, and perhaps proceeds from some particles of feld-spath accompanying the Gadolinite.

Note on fluated Alumine and Phosphate of Copper.

M. KLAPROTH has completed the analyses of the fluated alumine, named *Chrysolite of Greenland*. It consisted of $23\frac{1}{2}$ parts of alumine, 36 of soda, and $40\frac{1}{2}$ of fluoric acid and water of crystallization. He considers the discovery of soda in stony fossils as particularly remarkable. It is at least the first instance of so great a quantity of this alkali being found in a stone, or rather in an insoluble salt.

Cit. Vauquelin has repeated the analyses of fluated alumine. The result was, alumine 21, soda 32, fluoric acid and water of crystallization 47. This chemist observes, that in the

the alumine were discovered five parts of a substance having an earthy appearance, but which does not form alum, and is apparently insoluble in the acids. He conceives this substance may be filix.

M. Klaproth has also discovered a native phosphate of copper, and is at present employed in analysing this new combination of the phosphoric acid.

*** The two last Articles were communicated by Letter from Klaproth to Vauquelin, Bulletin des Sciences, No. 45, Frimaire, IX.*

On a new Manner of preparing the Acetic Acid. By CIT. BADOLLIER, Apothecary at Chartres.

THE process of Cit. Badollier consists in distilling a mixture of equal parts of sulphate of copper and acetite of lead, on the sand bath, in a glass retort adapted to a receiver.

To examine the produce, Cit. Badollier made use of the pneumato-chemical apparatus. During the operation, he obtained only a small quantity of elastic fluid, which he found to have proceeded from the dilated air contained in the vessels. This air, tried by the usual tests, afforded no indication of carbonic acid. As he presumed that his acetic acid might contain a portion of sulphuric acid, he tried it by a solution of the muriate of barytes, which did not cause any precipitate.

It must be observed, that the distillation is speedily made, and requires a very moderate heat; and the acid thus obtained possessed no empyreumatic smell; that the quantity, as well as quality, were equal to those obtained by the distillation of the acetite of copper; and that there was not, as in the old process, a portion of decomposed acid, communicating a disagreeable smell to the whole product. On account of the time and fuel saved, the price of the acid prepared by this new process, is to that obtained by the distillation of the acetite of copper, as 1 to 4.

Annales de Chimie, XXXVII. III.

On the Phosphorescent Properties of the Medullary Substance of the Brain and Nerves, extracted from a Memoir of CIT. CABARRIS, read before the National Institute.

IT is well known that phosphorus is derived from animal matter. It is also indeed found in the mineral kingdom; but it may be questioned whether its origin, like that of calcareous earths, may not always be referred to animal recrements; that at least which is the direct product of these recrements, may be considered as the immediate effect of sensitive

sensitive life, as a result of the changes which the animal solids and fluids are susceptible of undergoing, or as one of the simple substances which they possess the peculiar property of assimilating. In the bodies of animals which are undergoing decomposition, phosphorus appears to enter into a slow combustion. Without producing real flame, without at least the power of igniting combustible bodies in its immediate vicinity, it becomes luminous, and throws out flashes of light amid the surrounding darkness, sufficiently vivid to afford some real foundation for the existence of those phantoms, which people are at once desirous, and afraid of seeing, in the neighbourhood of cemeteries. The brain and its appendices, or rather the nervous system in general, appears to be the peculiar reservoir of phosphorus; for it is the incipient decomposition of the cerebral pulp which gives rise to those phosphorescent lights which, during the darkness of the night, are frequently observed in anatomical theatres; and it is chiefly around the brain exposed by being deprived of the natural coverings, or the fragments of nervous matter lying on the tables, that they are perceived. A variety of observations have led me to conclude that the quantity of phosphorescent matter developed after death, bears some proportion to the activity of the nervous system during life. I think I have noticed that the brains of persons who have died of diseases marked by a strong augmentation of this activity, gave a greater, as well as a more vivid light. Those of maniacs are peculiarly luminous; those of dropical and leucophlegmatic subjects much less so.

Combustion of Metals by Galvanism.

PROFESSOR Tromsdorf presented to the Electoral Society of useful Literature at Erfurth, April 8th, a Volta's pile, and exhibited in the presence of the Prince Bishop de Constanz, some experiments, part before known, and part new.

The pile of Professor Tromsdorf was built up of 180 pair of plates, of zinc, copper, and wetted paper.

The shock which this pile was capable of giving was very strong, and the spark considerable.

Professor Tromsdorf, after having shewn the production of hydrogen, and oxygen gas, the oxidation and de-oxidation of metals, &c. exhibited the combustion of metals, both perfect and imperfect; a fact hitherto unknown to us.

A leaf of fine gold, after having been affixed to the zinc side of the pile, took fire, and burnt with a crackling noise, when the wire of the copper side was brought in contact with it.

A leaf of fine silver took fire in a similar manner, and burnt with a green flame; brass burnt reddish brown; laminated copper, emerald-green; zinc, whitish-blue; tin, reddish-white, &c.

In order to insure success in the exhibition of these experiments, Professor Tromsdorf remarks, that the metals should be rolled out very thin, though he does not doubt that a larger pile would be capable of inflaming larger masses.

To prove that the accension of the perfect metals is a true oxidation, the experiments may be performed in a hollow glass sphere; the oxide will then adhere to the sides of the glass, and may be collected.

Professor Tromsdorf is now engaged in the construction of a larger pile, consisting of 5 or 600 pairs of plates.

Allgemeine Literatur Zeitung LXXXIV. July 16, 1801.

THE combustion of metals by galvanism has also been effected in a somewhat different way by Fourcroy in France. An account of this experiment was given in the *Moniteur* about two months ago; but as the set to which I have access appears to be defective for want, as it should appear, of this very Paper, I must, for the present, give the particulars from recollection. A pile of plates of zinc and silver six inches, or more in diameter, and consisting of no more than six plates of each metal, was constructed in the usual manner. It gave little or no perceptible shock, but the galvanic stream, when passed through fine wire, ignited and dispersed it. The most remarkable fact in this exhibition is, that the same surface formed into a pile with more reiterations; as for example, when the plates were cut into four, and constituted twenty-four repetitions, gave strong shocks, but did not exhibit the combustion of wire. From this curious incident we derive a law of galvanism before unknown, which is not only in itself very striking, but will tend to improve our knowledge of the electric shock, if electricity be the same as galvanism, or to mark their differences more effectually, if the contrary position should hereafter be rendered probable.

Galvanic luminous Signs of the two Electric Powers.

MR. Cruickshank of Woolwich, has favoured me with an account in conversation, of the flash of light attended with noise, which he observed many months ago, and mentioned to me formerly in a letter, from which an extract is given at page 80 of the present volume. The apparatus made use of to produce the galvanic current is there described, namely, two troughs, consisting each of 120 pair of plates of zinc and silver, folded together all round the edge. If the communication be made from one end of this system to the other, by means of a wire having one of its extremities plunged in the fluid, so as to communicate with the zinc face, and the other extremity of the wire be applied to the fluid at the end to which

which the surfaces of the silver plates are opposed, a flash or brush of light, visible in clear sun shine, is seen upon the surface of the water at the moment of contact or immersion; attended with a noise exactly similar to that of plunging a red hot wire in water, and accompanied with an ebullition of the same kind, as is produced by such an immersion. The length of the brush is about half an inch, and the noise may be heard across a room. On the contrary, if the permanent connection, or immersed end of the wire be at the silver end, or that extremity which would separate hydrogen from water, and the application or contact be made to the fluid at the zinc or oxygen end, nothing is seen but a small globular speck of light, scarcely-visible unless the machine be very powerful at the same time that there is very little effervescence, and no noise at all.

From my experiments at page 184 of the 4th Volume of this Journal, which have since been repeated and varied by numberless operators, it is determined that the zinc or oxygen side imparts the plus state of electricity, and the other side the minus. The point of the wire was therefore plus when it gave out the luminous brush, and minus when the fluid was silently conducted. This property of the two electricities was long ago shewn by Franklin, and from some new facts respecting the operation of low points, was applied by myself to the construction of an instrument called the Distinguisher of Electricity, of which a drawing was published in my Introduction to Natural Philosophy about fourteen years ago. The course of electricity from a battery, that is to say, of a large quantity at a low or moderate intensity, along the surface of fluids or moist substances, was the object of a series of experiments many years ago by Dr. Priestley, which he has published in his History of Electricity. Dr. Wollaston has communicated some very effective experiments to the Royal Society, by which the identity of the electrical and galvanic powers appears to be placed beyond controversy, an account of which will soon be laid before the reader.

Mr. Closs's Improvements in Wind Instruments.

Fig. 1.

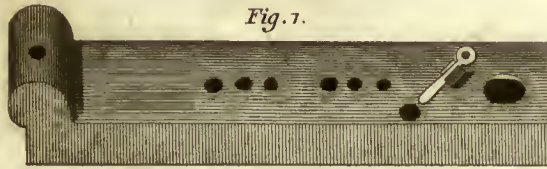


Fig. 3.

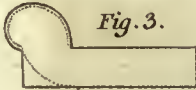


Fig. 2.



Fig. 4.



Fig. 5.

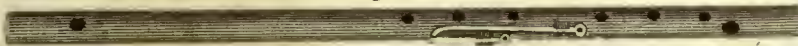


Fig. 6.

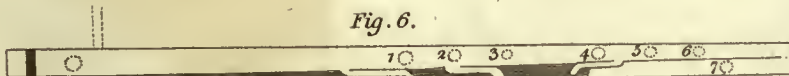


Fig. 7.



Fig. 8.

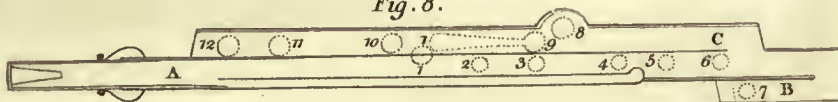
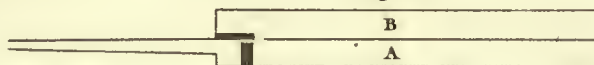


Fig. 9.



Fig. 10.



Mr Walker's freezing Procefs.

Fig. 1.

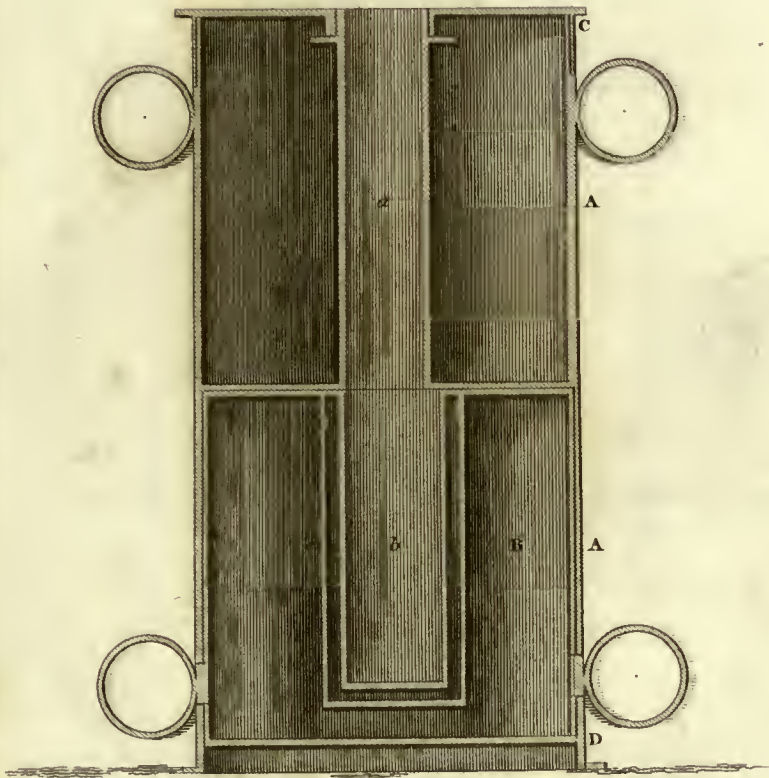
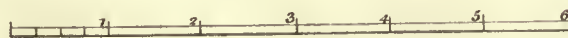
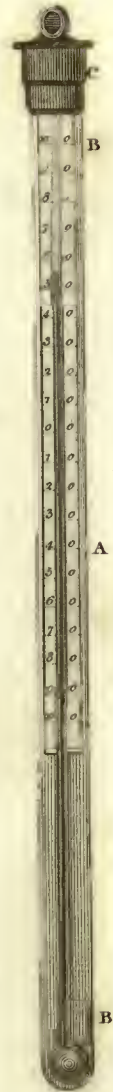


Fig. 2.



Scale of Inches.



A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

OCTOBER, 1801.

ARTICLE I.

New Theory of the Constitution of mixed Aeriform Fluids, and particularly of the Atmospheres.
By Mr. JOHN DALTON.

TO MR. NICHOLSON.

SIR,

AT the period when the mechanical properties of the air were first ascertained, by the invention of the air-pump, &c. the atmosphere was understood to be a homogeneous fluid, exclusively possessing the property of permanent elasticity. Experiment determined that the elastic force of air was accurately as its density, in a given temperature. This fact afforded a *datum* from which the law of repulsion of the corpuscles of air could be calculated. Accordingly Newton demonstrated, (*Principia Prop. 23, B. 2.*) that an homogeneous elastic fluid possessing a force in proportion to its density, is constituted of particles repelling each other by a force decreasing directly as the distance of the centres of the particles from each other. This result is, I believe, universally allowed as unquestionable. Modern chemistry however, demonstrates that the atmosphere is *not* an homogeneous fluid; it is constituted of several elastic fluids, possessing distinct properties; but as they all agree in elasticity, that is, in their particles repelling each other by a force decreasing as their distance, it seems to have been tacitly admitted by modern philosophers, that the same law of repulsion takes place between two particles of different fluids, as does between two particles of the same fluid. This admission, I conceive, has been a most unfortunate one, and the cause of

much embarrassment and uncertainty to the chemist and philosopher, which otherwise needed not to have taken place.

There may be *four* suppositions in respect to the affections of the particles of one elastic fluid towards those of another:

1. Particles of one elastic fluid may repel those of another with *the same* force as they repel those of their own kind.
2. The particles of one may repel those of another with forces *greater* or *less* than that exerted upon those of their own kind.
3. The particles of one may possess no repulsive (or attractive) power, or be perfectly inelastic with regard to the particles of another; and consequently the mutual action of such fluids, or the action of the particles of one fluid on those of the other, will be subject to the laws of inelastic bodies.
4. The particles of one may have a chemical affinity, or attraction, for those of another.

Let us now consider the consequences resulting from each of those suppositions: according to the first, if there were two gases *A* and *B*, the particles of which were of equal specific gravity; then if *m* measures of *A* were mixed with *n* measures of *B*, in the pneumatic apparatus, and under atmospheric pressure equal to 30 inches of mercury, the two gases would occupy *m + n* measures of space, and would remain in such circumstances as they were at any time placed; that is, either intermingled or separate: but if *A* was of less specific gravity than *B*, *A* would occupy the upper part of the vessel, and the volume of the compound continue the same as before. The pressure on each particle of the mixture would be the same in both cases, and equal to 30 inches of mercury.

According to the second hypothesis, if the particles of *A* had a greater or less repulsion to those of *B* than to their own, still *m* measures of *A* with *n* measures of *B*, would occupy *m + n* measures of space, and the pressure on each particle be the same, and equal to 30 inches of mercury. But the fluids could not be mixed, unless the repulsive powers of their particles were as the cube roots of the specific gravities of those particles; or, which amounts to the same thing, unless the two fluids were of equal specific gravities.

On the third hypothesis, if *m* measures of *A* be mixed with *n* measures of *B*, the two will in this case likewise occupy *m + n* measures of space; but the two fluids, whatever their specific gravities may be, will immediately or in a short time, be intimately diffused through each other, in such manner that the density of each, considered abstractedly, will be uniform throughout; namely (calling the density of the compound, unity) that of *A* will be $\frac{m}{m+n}$, and that of *B* = $\frac{n}{m+n}$; because, meeting with no repulsion but what arises from particles of *the same kind*, they will diffuse themselves through any given space, occupied by a very rare medium, in the same manner as they would do in a vacuum, each particle being impelled as far as possible from its neighbouring particle; only the diffusion of each may be a little retarded by the other. The pressure upon any one particle in this case will not be as the density of the compound, as before; but as the density of the particles

of its own kind: that is, the pressure upon a particle of A will be equal $\frac{m}{m+n}$. 30 inches of mercury; and that upon a particle of $B = \frac{n}{m+n}$. 30 inches; those pressures arising solely from the particles of their own kind.

On the fourth hypothesis, where two elastic fluids meet, whose particles, instead of repelling, attract one another; that is, the particles of A attract those of B : then, two or more particles coalescing, a compound will be formed possessing peculiar properties. Here, if the compound happen to be an elastic fluid in that temperature, m measures of A and n measures of B , will occupy a space less than $m+n$ measures, and the new fluid be of greater specific gravity than the other two: this at least would most probably be the case.

To understand these deductions from the preceding hypotheses may perhaps require the aid of mathematical reasoning; but I think they will be mostly pretty obvious to such as are in some degree conversant in these subjects: I shall therefore proceed to the application; that is, to consider which of those hypotheses will best explain established facts. When two gases of very different specific gravities are confined in the same vessel, for instance the oxygenous and hydrogenous, they are ever found to be jointly diffused through the whole capacity of the vessel, disregarding the laws of hydrostatics. Again, with respect to elastic vapours from liquids; aqueous vapour under 212° , cannot possibly support a pressure equal to that of the atmosphere; vapour from alcohol requires 175° , and that from æther 100 or 105° , in order to be formed at all under a pressure of 30 inches of mercury; yet we see all these liquids evaporate daily in the open air, and at low temperatures. It is impossible to reconcile the first of these facts to either the first or second hypothesis; as for the second fact, we attempt to reconcile it to them by another hypothesis; namely, that one or all of the gases constituting the atmosphere, act as a general menstruum, and dissolve liquids indiscriminately in greater or less degree. But this explanation is not without some difficulties; for the specific gravity of the gases is *diminished* by this process, instead of being *increased*, and heat is absorbed by the liquid dissolved, just the same as if it were changed into a distinct elastic fluid.

If we adopt the *third* hypothesis, the explanation of both these facts, and indeed of every other, becomes at once easy and perfectly intelligible. The intimate mixture and diffusion of gases of whatever specific gravity, is a necessary consequence: the vapour of water, and of every other fluid which does not chemically unite to the azotic and oxygenous gases of the atmosphere, nor to one another, may exist as so many distinct fluids in every temperature of the atmosphere, and without any regard to its pressure on the surface of the earth, being totally uninfluenced by any other pressure than that arising from the weight of their own particles: in short each vapour, in regard to pressure, is in the same circumstances as if it were the only elastic fluid constituting the atmosphere.

Many other facts which are difficultly explicable upon any other hypothesis, are readily understood upon this. When a portion of atmospheric air is subjected to a solution of

fulphuret of pot-ash, the oxygenous gas in a short time is almost wholly absorbed: now it is not easy to conceive how any chemical affinity should operate upon a body at the distance perhaps of 12 inches or more. The fact is, the sulphur absorbs the oxygen in contact with it; and as the remainder of the gas expands to fill up the vacuity, it is successively absorbed in the same way. A similar absorption takes place, when a quantity of any gas containing aqueous vapour is confined along with sulphuric acid, or dry, deliquescent salts; the vapour is absorbed, and the mass is diminished in elasticity, notwithstanding the supposed chemical affinity of the gases for water.

Lavoisier's description of the atmosphere therefore appears to be too limited, when he says, "Our atmosphere is a compound of all the fluids which are susceptible of the vaporous or permanently elastic state, in the usual temperature, and, under the common pressure:" this last limitation should be omitted. The general atmosphere seems to be a compound of four fluids principally, or four particular atmospheres; *azotic gas*, which at a medium presses the earth's surface with a force equal to 21.2 inches of mercury; *oxygenous gas*, which is usually equal to 7.8 inches; *aqueous vapour*, which varies from 1 inch to .1, or less, according to climate and season; and *carbonic acid gas*, the pressure of which may perhaps be equal to $\frac{1}{2}$ inch. Those gases and vapour press *separately* on the surface of the earth; and any one of them may be withdrawn, or another added to the number, without materially disturbing the rest, or any way affecting their density.

The above doctrine necessarily requires the force of vapours from any fluid to depend *solely upon temperature*, and consequently to be the same in any gas as in an exhausted receiver. This I had proved to be the fact with several fluids, before this theory occurred to explain the reason. I intend to give some account of the experiments relating to this subject, and to evaporation in general, in a volume of *Memoirs of the Manchester's Literary and Philosophical Society*, now in the press; together with some experiments on the expansion of gases by heat, controverting certain results of Guyton and Du Vernois, in the *Annales de Chimie*, Vol. I.

The very important changes in our views of facts in chemistry, meteorology, &c. which the principles here advanced will make if adopted; and the striking manner in which they appear to me to resolve what have justly been considered as some of the greatest difficulties, must be my apology for desiring these observations to be communicated as soon as convenient.

I am,

With respect, &c.

JOHN DALTON.

Manchester, Sept. 14, 1801.

On

II.

On the Opinion or Inference that the Chemical Actions of Light and Heat are the same.

By Mr. R. HARRUP.

To Mr. NICHOLSON.

SIR,

THE result of some experiments which I made in 1797 and 1798, on the effects of light upon the mercurial oxides, and which were published last year in the 5th volume of the London Medical Review and Magazine, was,—That the red oxide of mercury, red nitrated mercury, and calomel, are reduced by exposure to the rays of the sun.

That this effect takes place when the oxides are inclosed in glass tubes hermetically sealed, and that the two former are intirely reduced, the tubes containing them becoming incrustated with quicksilver, and the latter partially, &c. Struck with the similar effects of light and heat in these instances, I put the question, if the light which is present during the reduction of calcined mercury in the ordinary way, has not some share in producing the effect? I did not know at that time, nor indeed till very lately, that any doubts were entertained of the action of light independent of heat; and it was only a few weeks ago, when looking over the 2d volume of your interesting Journal, that I became acquainted with the opinion of Count Rumford on the subject, “That all the visible changes produced in bodies by exposure to the action of the sun’s rays, are effected, not by any chemical combination of the matter of light with such bodies, but merely by the *heat* which is generated or excited, by the light which is absorbed by them,” seems to me to be a conclusion not warranted by experiment.

Into a very transparent glass tube I put ten grains of red oxide of mercury, spread out thin on a slip of window glass. To one half the direct rays of the sun were admitted through the tube, and the light excluded from the other half by means of a case of pasteboard, and two slices of cork, which fitted the tube exactly. That portion to which the light was admitted began to acquire a deeper colour almost instantaneously; some hours after, it had become of a dark dirty brown colour, with somewhat of a leaden lue. Upon examination in a strong light, with a lens the whole surface was found to be covered with small metallic globules. The effect was intirely superficial, for at the smallest depth below the surface the oxide was without change.

After exposure for three days no farther change took place, excepting that the colour was become somewhat deeper, and the mercury, as it was revived, sublimed and adhered to the upper side of the tube, which was, at least, half an inch distant. The portion of the oxide from which the light was excluded had suffered no change, excepting a very thin stratum of it at the end of the glass next the slice of cork, which was used as a stopper.—

This

This part was become quite black, but contained no globules. This was the more surprising, as I had not observed any thing similar in my former experiments. By accurate examination it was found, that the rays of light had been admitted to this part of the oxide through the slip of window glass, which was of a *greenish cast*. Having no prism by me, I had not an opportunity of ascertaining the effect of different coloured rays.

As some of the Count's experiments seem to indicate, that moisture has some concern in producing the changes, and having myself frequently observed a small quantity of vapour exhale, immediately on exposure to the rays; I endeavoured to ascertain the truth by experiment. For this purpose I introduced into a very dry glass tube, a small quantity of the red oxide made perfectly dry, and enclosed it in such a manner as totally to prevent the access of air and moisture. By exposure to the rays the same changes took place, and the only difference I could perceive was, that in this the oxide acquired its deepest colour in less time than in the former experiment; moisture then, we may conclude, has no effect in the reduction of mercury.

From these experiments it appears, that light acts intirely on the surface. This alone seems to be a material objection to the conclusion of the Count, for we cannot well suppose, that so intense a heat as is necessary for the reduction of the oxide, should produce its effects merely on the surface, and no where else. However, there are other objections which appear to be insuperable. If the effect be produced by the heat alone which the action of light occasions, it must necessarily follow, that the metallic oxides in general, when placed in contact with inflammable bodies, will be reduced by mere exposure to the sun's rays. To know whether this is actually the case, I made choice of the oxides of lead for the subject of experiment, on account of their well known property of being easily revived by caloric, when mixed with inflammable bodies. Small quantities of minium, and of acetite of lead, were separately exposed on glass to the direct rays of the sun for several hours. No change was produced, except that the water of crystallization had exhaled from the latter. Next day a small quantity of charcoal was thinly sprinkled on each, and gently pressed down. After exposure to the rays the whole day, not the smallest change was effected; they were then put into two small cylindrical glass vessels, and alcohol poured over them to the depth of half an inch, and again exposed on the third day till the spirit was intirely evaporated, but without any alteration whatever.

May we not therefore conclude that light acts independent of heat.

I am, SIR,

Your's respectfully,

ROBERT HARRUP.

Cobham, Surry, Sept. 1, 1801.

P. S. I cannot neglect this opportunity of giving the result of some experiments made three years ago, on the production of gas by freezing water, and of which I was reminded at present by reading Dr. Priestley's communication in the 4th volume of your Journal.

If

If water from which the air is extricated by long continued boiling, be shut up in glass vessels intirely filled with it and then frozen, they will burst by the expansion; and numerous air bubbles will be diffused throughout the ice. The same effect takes place if fresh distilled water is used. If water, warm from the still, be inclosed in exhausted bottles which will contain three or four times the quantity, and alternately frozen and thawed a great number of times, a quantity of gaseous fluid will be produced, equal at least in bulk to the water employed. What the nature of this gas is I have not yet been able to determine, as the quantity I procured was lost by accident; I only know, that it did not explode by the contact of a lighted taper. During the freezing and thawing several curious phænomena took place. Upon the whole, the subject is highly deserving attention, and it seems surprizing that it has not been thoroughly investigated long before this time.

R. H.

III.

*Memoir on a Method of House Painting with Milk; by ANTONY ALEXIS CADET-DE-VAUX.**

I PUBLISHED in the *Feuille du Cultivateur*, but at a time when the weight of public misery absorbed the thoughts of every individual, a process for painting, of singular œconomy, which the misfortune of the times compelled me to substitute, instead of painting in distemper.

One of my neighbours was repairing his country house, in which he employed some old wood work that necessarily required to be painted; but the universal want of every article, and more particularly of money, for it was in the time of the maximum and assignats, did not permit him to go to the expence. The amount of the colour would have been four hundred francs. In this situation he consulted me, and fortunately he applied to the proper person: for I was employed painting a hot-house in my garden, at the time he called to converse with me. He found me painting without distemper and without oil; he expressed his uneasiness about his four hundred francs, on which head I soon removed his anxiety, by assuring him that I would reduce the sum to a tenth part. Part of my hot-house had been painted in the morning: he passed the palm of his hand over this first layer, but I desired him to rub it with his great coat. It resisted like varnish. He demanded my secret; and as I keep no secrets of this nature, but on the contrary am desirous of publishing every thing which can be of value in different branches of œconomy, I gave him my process.

As I am not dextrous with the brush, I sent the next day for a painter from Franconville to paint the cieling and floor of my library.

* Decade Philosophique, No. 29. An. IX.

My paint smelled neither of size nor of oil. It is a miserable watery composition, said the painter. He seemed to do me a favour in condescending to use it. I had taken care not to shew him the experience of the day before. He began with the cieling; I presented him with another pot of yellow colour for the floor. While the floor was painting the, cieling became dry, and gave him an opportunity of judging of the solidity of this kind of painting. He immediately saw and admitted its superiority over the method in distemper; a single coat, or even several of which will not stand the trial of being rubbed with a coarse woollen cloth, without coming off. He was struck with astonishment, made his apology, and concluded by asking for my secret, which I immediately gave him, and which he has since turned to good account.

At the present moment I am about to paint it again, or rather I am desirous of making another trial to fix the process of this method, more especially that of substituting it in the place of oil painting.

Let us now proceed to the method itself.

Take of skimmed milk a pint, which makes two pints of Paris, or nearly two quarts English; fresh flaked lime, six ounces, (about $6\frac{1}{2}$ oz. avoirdupois); oil of caraways, or linseed, or nut, four ounces; Spanish white, (whiting) three pounds: put the lime into a stone ware vessel, and pour upon it a sufficient quantity of milk to make a mixture resembling thin cream; then add the oil a little at a time, stirring it with a small spatula; the remainder of the milk is then to be added, and lastly, the Spanish white. Skimmed milk in summer is often clotted, but this is a circumstance of no consequence to our object; because the contact with the lime soon restores its fluidity. But it must on no account be four, because in that case it would form with the lime a kind of calcareous acetite, capable of attracting moisture.

The lime is flaked by dipping it in water, out of which it is to be immediately taken, and left to fall in pieces in the air.

The choice of either of these oils is indifferent; nevertheless for white paint the oil of caraways is to be preferred, because colourless. The commonest oils may be used for painting with the ocras.

The oil when mixed in with the milk and lime disappears, and is totally dissolved by the lime, with which it forms a calcareous sope.

The Spanish white is to be crumbled, or greatly spread on the surface of the fluid, which it gradually imbibes, and at last sinks; at this period it must be well stirred in. This paint may be coloured like distemper (or size colour) with levigated charcoal, yellow ochre, &c.

And it is used in the same manner:

The quantity here prescribed is sufficient for the first coat of six toises, or 27 square yards English.

The price of this quantity amounts to nine sols, which reduces the price of the square toise to one sol, six deniers, prime cost.

The

The preceding instructions have been sufficient for most of those who have made use of this painting; but let us reason upon it, and discuss the theory which has guided me in the choice and combination of these ingredients:

In the first place, let us speak of painting in distemper. This is done, as every one knows with size, to which is added Spanish white, or ochre.

Its inconveniencies are numerous.

In the first place, it comes off by the slightest friction; so that for the most part it is an inconvenience to suffer the clothes to come in contact with the walls and passages painted in this manner.

It is true that this inconvenience is remedied by using stronger size, and laying on a greater number of coatings; but in this case, in the course of time, in dry exposures, the painting peels off, and is detached.

It is no trifling consideration, that three coatings in distemper will cost thirty fous, or near seven times as much as our paint of three coatings, if that were necessary.

The consequences where the exposure is moist are still more pernicious. The glue or size which forms the basis of distemper becomes moist, and ferments; like every other animal glue it partly passes to the state of acidity; it ceases to be glue, and is no longer adhesive; it becomes a fluid and acid water, which speedily acquires the putrid state. It is more particularly at the time of thaw that this effect of moisture becomes very sensible, and then the paint of this description is seen to flow in streaks along the walls and wood work. The earth and ochre becoming separated from the gelatinous substance, are then abandoned to their pulverulent state, and the paint falls off.

This disposition of animal gluten to attract moisture from the air, and to ferment, explains the reason why in process of time painting in distemper emits a smell, unless it be varnished, which partly remedies those inconveniencies; but at the same time it must be recollected, that it adds to the expence.

We must also remark, that the mixing of the size colour requires a fire, and demands the expence of workman's time during part of the morning for its preparation: and that lastly, this paint will not keep, but frequently changes in the course of twenty-four hours, particularly in stormy weather.

On the other hand, let us consider and compare our method of painting.

Skimmed milk has lost its butyraceous part, but it keeps its cheesy part or curd, of which milk contains more or less, at a medium about one fourth in a state of strong gelatine; consequently there is about a pound in the Paris pint, or four pounds of milk, which is our proportion. I say in the state of gelatine, for the caseous part not being soluble in water is not gelatine, but it is glue.

We have therefore one pound of glue, which deserves the name of strong glue, in preference to the glues which are so called, and this is appropriated to give body to our colours; but this glue of milk has great advantages over the common animal glue.

The cheefy part retains a portion of humidity, a principle which gives it elasticity. For example, take cheefe of Gruyeres; let it be ever so dry, it is still elastic; it may be rasped, but cannot be pounded.

This portion of humidity which the cheefy part retains is sufficient for its constitution. It does not therefore attract the moisture of the atmosphere, and the pigment of which it forms the base, does not become crumbly in dry elevated well-aired places, neither does it become soft in low and humid exposures.

One of the properties of this paint is, that it may be kept for whole months, and requires neither time nor fire, nor even manipulation; ten minutes are sufficient to prepare enough to paint a whole house. There probably is formed in this mixture a combination, which in that case would add much to the solidity of the painting, namely, that of the Spanish white with the oil. The putty of the glaziers, which becomes so solid that it is almost impossible to detach it, is nothing but this mixture, and so likewise is the lute of the chemists. It may easily be conceived, how much this mastic must add to the solidity of the colour in which it is dissolved.

This advantage is attended with other valuable conveniencies. A bed room may be inhabited the very night after it has been painted, because this paint becomes dry in an hour, and the oil which enters into the composition loses its smell and character of oil by its combination of lime, which forms a true soap; the smell of this colour when fresh laid is saponaceous; the apartment previous to its becoming dry smells like a laundry.

A single coat is sufficient for places which have been before painted. It is only necessary to lay on two when the spots of greafe repel the first coating; in this case they should be washed off with strong lime water or soap lees, or scraped.

New wood requires two coatings. One coat is sufficient for a staircase, passage, or cieling.

The hot-house I have spoken of has its door hung within: the door-sill is therefore exposed to all the injuries of the air; but notwithstanding this and the continual friction, the traces of the brush are still visible, and the paint has not undergone the slightest alteration.

I have since given to this method of painting a much greater degree of solidity; for my aim has been not only to substitute it instead of painting in distemper, but likewise of oil paint.

Milk Paint containing Resinous Matter.

For out-door work I add to the proportions of the paint,

Slaked lime,	- - -	2 ounces
Oil,	- - -	2 ounces
White Burgundy pitch,	- - -	2 ounces

The

The pitch is to be melted by a gentle heat in the oil, and then added to the smooth mixture of milk and lime. In cold weather this mixture ought to be warmed, in order that it may not occasion too speedy cooling of the pitch, and to facilitate its union with the milk of lime. This paint has some analogy with that known by the name of encaustic.

I have employed this composition for outside shutters, formerly painted with oil.

The common method of painting in oil, though thought to be so durable, participates strongly in the inconveniencies of painting in distemper; that is to say, it shrinks, cracks, scales off, and is separated; with this difference, that the flakes preserve some points of adhesion. But is not the less true, that the wood remains uncovered, and exposed without defence to the action of the air.

For the sake of neatness, it is proper to rub down the surface with sand stone, or pumice stone, before the first coating is laid on.

Time, which destroys every thing, will destroy this paint; but I do not in theory see any reason why the adhesion of the resinous milk paint should break, or that it should crack and scale off like the painting in oil; of which the base is ceruse, a preparation of lead, which renders the oil drying, and disposes this kind of paint to crack. This metallic substance has also the inconvenience of becoming coloured in the air by the action of hydrogen gas; so that white paint becomes yellow, and is frequently covered with black spots by a de-oxidation assumed by the oxide of lead. The exhalations of a drain or necessary, or the vicinity of a stable, are sufficient completely to blacken painting of lead colour with oil.

There is no lead in the paint of resinous composition with milk, consequently this inconvenience is not to be feared. The air cannot change the colour of this painting, as I am convinced by experience in my hot-house; and every building of this description is exposed to the exhalation of fermenting vegetable substances.

I need not mention the unwholesome smell of oil painting, nor the long time it continues. In country houses which are not inhabited during winter, it is found, to remain even for several years successively. The dangers of the exhalation of this kind of paint are frequent, and but too well known.

Our painting with resinous composition and milk, notwithstanding the oil and the Burgundy pitch which enter into its composition, emits no other smell than that of turpentine*; a smell which is speedily dissipated in a day or two, besides which it is used only for outdoor work.

The reasons are stronger in favour of substituting either of these new modes of painting, to that which is called *badigeon*†. This pigment consists of quick-lime, yellow ochre, and water; it lasts very well within-doors, but when exposed to the air it soon disappears. This mixture has no consistence; its adherence to stone is so weak, that the rain carries it off, and leaves no trace in two or three years.

* It is well known that Burgundy pitch is the residue of distillation of turpentine with water, an operation for the purpose of separating the volatile oil.

† We have no appropriate name in English for this method.

This method of painting besides costs ten sols the toise; but as we have before observed, the painting in distemper with milk does not cost more than one sol, six deniers, to which we must add a small matter for the expence of laying it on, as a man can cover a great number of toises in a day.

But I should prefer for these purposes the painting with the resinous composition and milk, using the lime and yellow ochre, with or without the addition of Spanish white. This paint would last twenty or thirty years without alteration; I know of nothing but the nitrification of the walls which could injure it, and this is a slow process.

I have spoken much to the disadvantage of painting in oil and in distemper, and I have pleaded the cause of my new process; but the theory on which I founded my preparation, and the experience I have had, speak strongly in favour of my conclusions.

I see only one objection, namely, the difficulty of procuring milk. This is far from being a difficulty, for there is more want of buyers for milk, than milk for the buyers. In the season of fruits in France there is scarcely any demand for milk, and in many places they are but very indifferently skilled in making cheese or butter. The summer is the season for painting. The consumption of milk at that time will even be advantageous to the farmer, and in no respect detrimental to those who consume it as food. It may also be observed, that for this purpose the milk may be brought from a much greater distance than it could for consumption as food, for if half the lime be added at the place where the milk was produced, it may then be carried without becoming sour or clotting, in a perfect state to the place of its destination. Little objection can be made in the country as to the scarcity of milk.

The political objection, that the house painters would by this practice be reduced to beggary, would also be a great objection with me to the process, if I were not convinced from numberless instances, that the increased consumption of a cheap thing more than compensates for the difference of charge by the abundance of employ it gives. Out of a hundred persons who would paint their apartments at twenty-five francs, there are scarcely five who would paint, if the price were fifty crowns. Those who hold their dwellings only for a short term, would adopt this useful and wholesome process when they knew its cheapness; so that upon the whole we might reasonably infer, that twice the number of painters might be employed in the workmanship, when the materials could be afforded so much better and cheaper.

The facility with which this work may be performed, may also be of some advantage in preventing workmen from giving the law to their employers; for there are many operations of domestic employ which are much more difficult than this. The mixture may be brought to a still greater degree of perfection. Fat or tallow may be substituted instead of oil, and wax instead of Burgundy pitch.

I therefore invite those chemists who devote their exertions to the improvement of the arts, to attend to this method of painting, and fix the proportions; to ascertain whether

lime acts upon the colouring oxides or ochres; and, lastly, to render the process generally useful. By this union of effort the present valuable branch of the arts would be simplified, individuals would obtain considerable savings, our habitations would become more neat and cleanly, and particularly more healthy, as there is nothing which alters the purity of the air more than oil paint.

IV.

On the Mechanism of the Eye. By THOMAS YOUNG, M.D. F.R.S.*

I. IN the year 1793, I had the honour of laying before the Royal Society, some observations on the faculty by which the eye accommodates itself to the perception of objects at different distances. † The opinion which I then entertained, although it had never been placed exactly in the same light, was neither so new, nor so much forgotten, as was supposed by myself, and by most of those with whom I had any intercourse on the subject. Mr. Hunter, who had long before formed a similar opinion, was still less aware of having been anticipated in it, and was engaged, at the time of his death, in an investigation of the facts relative to it; ‡ an investigation for which, as far as physiology was concerned, he was undoubtedly well qualified. Mr. Home, with the assistance of Mr. Ramsden, whose recent loss this Society cannot but lament, continued the inquiry which Mr. Hunter had begun; and the results of his experiments appeared very satisfactorily to confute the hypothesis of the muscularity of the crystalline lens. § I therefore thought it incumbent on me, to take the earliest opportunity of testifying my persuasion of the justice of Mr. Home's conclusions, which I accordingly mentioned in a dissertation published at Göttingen in 1796, || and also in an essay presented last year to this Society. ** About three months ago, I was induced to resume the subject, by perusing Dr. Porterfield's paper on the internal motions of the eye; †† and I have very unexpectedly made some observations, which I think I may venture to say, appear to be finally conclusive in favour of my former opinion, as far as that opinion attributed to the lens a power of changing its figure. At the same time, I must remark, that every person who has been engaged in experiments of this nature, will be aware of the extreme delicacy and precaution requisite, both in conducting them, and in drawing inferences from them; and will also readily allow, that no apology is necessary for the fallacies which have misled many others, as well as myself, in the application of those experiments to optical and physiological determinations.

* Philof. Transf. 1801. † Phil. Transf. for 1793, p. 169.

† Phil. Transf. for 1794, p. 21. § Phil. Transf. for 1795, p. 21.

|| De Corporis humani Virtibus conservatricibus, p. 68.

** Phil. Transf. for 1800, p. 146.

†† Edinb. Med. Essays, Vol. IV. p. 124.

II. Besides the inquiry respecting the accommodation of the eye to different distances, I shall have occasion to notice some other particulars relative to its functions; and I shall begin with a general consideration of the sense of vision. I shall then enumerate some dioptrical propositions subservient to my purposes, and describe an instrument for readily ascertaining the focal distance of the eye. On these foundations, I shall investigate the dimensions and refractive powers of the human eye in its quiescent state; and the form and magnitude of the picture which is delineated on the retina. I shall next inquire, how great are the changes which the eye admits, and what degree of alteration in its proportions will be necessary for these changes, on the various suppositions that are principally deserving of comparison. I shall proceed to relate a variety of experiments which appear to be the most proper to decide on the truth of each of these suppositions, and to examine such arguments as have been brought forwards, against the opinion which I shall endeavour to maintain; and I shall conclude with some anatomical illustrations of the capacity of the organs of various classes of animals, for the functions attributed to them.

III. Of all the external senses, the eye is generally supposed to be by far the best understood; yet so complicated and so diversified are its powers, that many of them have been hitherto uninvestigated; and on others, much laborious research has been spent in vain. It cannot indeed be denied, that we are capable of explaining the use and operation of its different parts, in a far more satisfactory and interesting manner than those of the ear, which is the only organ that can be strictly compared with it; since, in smelling, tasting, and feeling, the objects to be examined come almost unprepared into immediate contact with the extremities of the nerves; and the only difficulty is, in conceiving the nature of the effect produced by them, and its communication to the sensorium. But the eye and the ear are merely preparatory organs, calculated for transmitting the impressions of light and sound to the retina, and to the termination of the soft auditory nerve. In the eye, light is conveyed to the retina, without any change of the nature of its propagation: in the ear, it is very probable, that instead of the successive motion of different parts of the same elastic medium, the small bones transmit the vibrations of sound, as passive inelastic hard bodies, obeying the motions of the air in their whole extent at the same instant. In the eye, we judge very precisely of the direction of light, from the part of the retina on which it impinges: in the ear, we have no other criterion than the slight difference of motion in the small bones, according to the part of the tympanum on which the sound, concentrated by different reflections, first strikes; hence, the idea of direction is necessarily very indistinct, and there is no reason to suppose, that different parts of the auditory nerve are exclusively affected by sounds in different directions. Each sensitive point of the retina is capable of receiving distinct impressions, as well of the colour as of the strength of light; but it is not absolutely certain, that every part of the auditory nerve is capable of receiving the impression of each of the much greater diversity of tones that we can distinguish; although it is extremely probable, that all the different parts of the surface exposed to the fluid of the vestibule, are more or less affected by every sound, but in different degrees and succession,
according

according to the direction and quality of the vibration. Whether or no, strictly speaking, we can hear two sounds, or see two objects, in the same instant, cannot easily be determined: but it is sufficient, that we can do both, without the intervention of any interval of time perceptible to the mind; and indeed we could form no idea of magnitude, without a comparative, and therefore nearly cotemporary perception of two or more parts of the same object. The extent of the field of perfect vision for each position of the eye, is certainly not very great; but it will appear hereafter, that its refractive powers are calculated to take in a moderately distinct view of a whole hemisphere: the sense of hearing is equally perfect in almost every direction.

IV. DIOPTRICAL PROPOSITIONS.

Proposition I. Phenomenon.

In all refractions, the ratio of the sine of the angle of incidence to the sine of the angle of refraction is constant. (Newton's Opt. I. Ax. 5. Smith's Opt. 13. Wood's Opt. 24.)

Scholium 1. We shall call it the ratio of m to $m \mp 1$, and $m \mp 1$, n . In refractions out of air into water, $m = 4$ and $n = 3$, very nearly; out of air into glass, the ratio is nearly that of 3 to 2.

Scholium 2. According to Barrow, (*Lect. Opt.* ii. 4.) Huygens, Euler, (*Conject. phys. circa prop. soni et luminis. Opusc. t. ii.*) and the opinion which I lately submitted to the Royal Society, (*Phil. Trans.* for 1800. p. 128,) the velocity of light is the greater the rarer the medium: according to Newton, (*Schol. Prop.* 96. l. i. *Princip. Prop.* 10. p. 3. l. ii. *Opt.*) and the doctrine more generally received, the reverse. On both suppositions, it is always the same in the same medium, and varies in the ratio of the sines of the angles. This circumstance is of use in facilitating the computation of some very complicated refractions.

Proposition II. Phenomenon.

If between two refracting mediums, a third medium, terminated by parallel surfaces, be interposed, the whole refraction will remain unchanged. (Newton's Opt. l. i. p. 2. *Prop.* 3. Smith. r. 399. Wood, 105.)

Corollary. Hence, when the refractions out of two mediums into a third are given, the refraction at the common surface of these mediums may be thus found. Let the refractions given be as $m : n$, and as $m' : n'$; then the ratio sought will be that of $m n' : m' n$. For instance, let the three mediums be glass, water, and air; then $m = 3$, $n = 2$, $m' = 4$, $n' = 3$, $m n' = 9$, and $m' n = 8$. If the ratios be 4 : 3, and 13 : 14, we have $m n' : m' n :: 39 : 56$; and, dividing by 36 — 39, we obtain 2.3 and 3.3 for m and $m + 1$, in *Schol.* 1, *Prop.* I.

Proposition

Proposition III. Problem. (Plate XIII. Fig. 1.)

At the vertex of a given triangle (CBA), to place a given refracting surface (B), so that the incident and refracted rays may coincide with the sides of the triangle (AB and BC.)

Let the sides be called d and e ; then in the base take, next to d (or AB), a portion (AE) equal to $\frac{n d}{n d + m e}$, or (AD) $= \frac{m d}{m d + n e}$; draw a line (EB, or DB) to the vertex, and the surface must be perpendicular to this line, whenever the problem is physically possible. When e becomes infinite, and parallel to the base, take $\frac{n d}{m}$ or $\frac{m d}{n}$ next to d , for the intersection of the radius of curvature.

Proposition IV. Theorem. (Fig. 2.)

In oblique refractions at spherical surfaces, the line (AI, KL,) joining the conjugate foci (A, I; K, L;) passes through the point (G), where a perpendicular from the centre (H) falls on the line (EF), bisecting the chords (BC, BD,) cut off from the incident and refracted rays.

Corollary 1. Let t and u be the cosines of incidence and refraction, the radius being r , and d and e the respective distances of the foci of incident and refracted rays; then

$$e = \frac{m d u u}{m d u - n d t - n t t}$$

Corollary 2. For a plane surface, $e = \frac{m d u u}{-n t t}$.

Corollary 3. For parallel rays, $d = \infty$, and $e = \frac{m u u}{m u - n t}$.

Scholium 1. It may be observed, that the caustic by refraction stops short at its cusp, not geometrically, but physically, the total reflection interfering.

Corollary 4. Call $\frac{m u u}{m u - n t}$, b , and $\frac{n t t}{m u - n t}$, c ; then $e = \frac{b d}{d - c}$, and $e - b = \frac{b c}{d - c}$; or, in words, the rectangle contained by the focal lengths of parallel rays, passing and repassing any surface in the same lines, is equal to the rectangle contained by the differences between these lengths and the distances of any conjugate foci.

Corollary 5. For perpendicular rays, $e = \frac{m d}{d - n} = m + \frac{m n}{d - n}$; or, if the radius be a , $e = \frac{m a d}{d - n a}$; and if d and e be given to find the radius, $a = \frac{d e}{m d + n e}$.

Corollary 6. For rays perpendicular and parallel, $e = m$, or $e = m a$.

Corollary

Corollary 7. For a double convex lens, neglecting the thickness, call the first radius g , the second b , and $e = \frac{n d g b}{d g + d b - n g b}$. Hence $n = \frac{d e}{d + e} \cdot \frac{g + b}{g b}$; and, for parallel rays, $e = \frac{n g b}{g + b}$, and $n = e \cdot \frac{g + b}{g b}$. If $g = b = a$, $e = \frac{n a d}{2 d - n a}$; and for parallel rays $e = \frac{n a}{2}$: calling this principal focal-length b , $e = \frac{b d}{d - b}$, as in Cor. 4; whence we have the joint focus of two lenses; also, $b = \frac{d e}{d + e}$.

Corollary 8. In a sphere, $e = m a \cdot \frac{d + a}{2 d - (m - 2) a}$, for the distance from the centre, and $b = \frac{m a}{2}$.

Scholium 2. In all these cases, if the rays converge, d must be negative. For instance, to find the joint focus of two convex, or concave lenses, the expression becomes, $e = \frac{b d}{b + d}$.

Corollary 9. In Cor. 3, the divisor becomes ultimately constant; and, when the inclination is small, the focus varies as $u u$.

Corollary 10. For parallel rays falling obliquely on a double convex, or double concave lens, of inconsiderable thickness, the radius being 1, $e = \frac{n t u}{2 (m u - n t)}$; which varies ultimately as the product of the cosines, or as $\frac{m + n}{n n} t + t^2$.

Scholium 3. In the double convex lens, the thickness diminishes the effect of the obliquity near the axis; in the double concave, it increases it.

Scholium 4. No spherical surface, excepting one particular case, (Wood, 155,) can collect an oblique pencil of rays, even to a physical point. The oblique rays which we have hitherto considered, are only such as lie in that section of the pencil which is made by a plane passing through the centre and the radiant point. They continue in this plane, notwithstanding the refraction, and therefore will not meet the rays of the collateral sections, till they arrive at the axis. The remark was made by Sir Isaac Newton, and extended by Dr. Smith, (Smith r. 493, 494;) it appears, however, to have been too little noticed. (Wood, 362.) The geometrical focus thus becomes a line, a circle, an oval, or other figure, according to the form of the pencil, the nature of the surface, and the place of the plane receiving the image. Some of the varieties of the focal image of a cylindrical pencil obliquely refracted are shown in Fig. 28.

Corollary 11. Hence the line joining the remoter conjugate foci, will always pass through the centre. The distance of the remoter focus of parallel rays will be expressed by

$f = \frac{m}{mu - nt}$; and the least circle of aberration will be at the distance $\frac{1 + n^2 - 2u^4}{(1 + uu) \cdot (mu - nt)}$, dividing the length of aberration in the ratio of the distance of its limits from the surface.

In the case of Cor. 10. $f = \frac{n}{2(mu - nt)}$.

Corollary 12. This proposition extends also to reflected rays; and, in that case, the line from the centre passes through the point of incidence.

Proposition V. Problem.

To find the place and magnitude of the image of a small object, after refraction at any number of spherical surfaces.

Construction. (Plate XIII. Fig. 3.) From any point (B) in the object (AB), draw lines to (C), the centre of the first surface, and to (D), the focus of parallel rays coming in a contrary direction: from the intersection of the second line (BD) with the tangent (EF) at the vertex, draw a line (EH) parallel to the axis, and it will cut the first line (BC) in (H), the first image of the point (B). Proceed with this image as a new object; and repeat the operation for each surface; and the last point will be in the image required. For calculation, find the place of the image by Cor. 5. Prop. IV, and its magnitude will be to that of the object, as their respective distances from the centre.

Corollary. If a confused image be received on any given plane, its magnitude will be determined by the line drawn from the preceding image through the centre of the last surface.

Proposition VI. Problem.

To determine the law by which the refraction at a spherical surface must vary, so as to collect parallel rays to a perfect focus.

Solution. Let v be the versed sine to the radius r ; then, at each point without the axis, n remaining the same, m must become $\sqrt{m^2 + 2nv}$; and all the rays will be collected in the principal focus.

Corollary. The same law will serve for a double convex lens, in the case of equidistant conjugate foci, substituting n for m .

Proposition VII. Problem.

To find the principal focus of a sphere, or lens, of which the internal parts are more dense than the external.

Solution. In order that the focal distance may be finite, the density of a finite portion about the centre must be equable: call the radius of this portion $\frac{1}{n}$, that of the sphere being unity; let the whole refraction out of the surrounding medium into this central part, be as

m to n ; take $r = \frac{\log. l}{\log. m - \log. n}$, and let the density be supposed to vary every where in-

versely as the power $\frac{1}{r}$ of the distance from the centre: then the principal focal distance from the centre will be $\frac{r-1}{2} \cdot \frac{m}{n-1}$. When $r=1$, it becomes $\frac{1}{2} (\text{H. L. } m - \text{H. L. } n)$.

For a lens, deduct one-fourth of the difference between its axis and the diameter of the sphere of which its surfaces are portions.

Corollary. If the density be supposed to vary suddenly at the surface, m must express the difference of the refractions at the centre and at the surface; and the focal distance, thus determined, must be diminished according to the refraction at the surface.

Proposition VIII. Problem.

To find the nearer focus of parallel rays falling obliquely on a sphere of variable density.

Solution. Let r be as in the last proposition, s the sine of incidence, t the cosine, and e the distance of the focus from the point of immersion. Then $e = \frac{w-t}{2-tw}$, w being

$$\frac{2}{r+1} \cdot (aA + bB + cC + \dots) + 2aA + 6bB + 10cC + \dots,$$

(where $r=1$)

$$\text{where } a = \frac{r-1}{r+1}, b = \frac{r-1}{3r-1}, c = \frac{r-1}{5r-3}, A = 1, B = \frac{1}{2}A, C = \frac{1}{4}B, D = \frac{1}{8}C.$$

But, when s is large, the latter part of the series converges somewhat slowly. The former part might be abridged if it were necessary: but, since the focus in this case is always very imperfect, it is of the less consequence to provide an easy calculation.

General Scholium. The two first propositions relate to well known phenomena; the third can hardly be new; the fourth approaches the nearest to Maclaurin's construction, but is far more simple and convenient; the fifth and sixth have no difficulty; but the two last require a long demonstration. The one is abridged by a property of logarithms; the other is derived from the laws of centripetal forces, on the supposition of velocities directly as the refractive densities, correcting the series for the place of the apsis, and making the sine of incidence variable, to determine the fluxion of the angle of deviation.

V. Dr. Porterfield has employed an experiment, first made by Scheiner, to the determination of the focal distance of the eye; and has described, under the name of an optometer, a very excellent instrument, founded on the principle of the phenomenon.*

* Edinb. Med. Ess. Vol. IV. p. 185.

But the apparatus is capable of considerable improvement; and I shall beg leave to describe an optometer, simple in its construction, and equally convenient and accurate in its application.

Let an obstacle be interposed between a radiant point (R, Plate XIII. Fig. 4,) and any refracting surface, or lens (CD), and let this obstacle be perforated at two points (A and B) only. Let the refracted rays be intercepted by a plane, so as to form an image on it. Then it is evident, that when this plane (EF) passes through the focus of refracted rays, the image formed on it will be a single point. But, if the plane be advanced forwards (to GH), or removed backwards (to IK), the small pencils passing through the perforations, will no longer meet in a single point, but will fall on two distinct spots of the plane (G, H; I, K;) and, in either case, form a double image of the object.

Let us now add two more radiating points, (S and T, Fig. 5,) the one nearer to the lens than the first point, the other more remote; and, when the plane which receives the images passes through the focus of rays coming from the first point, the images of the second and third points must both be double (*s, s'; t, t'*;) since the plane (EF) is without the focal distance of rays coming from the furthest point, and within that of rays coming from the nearest. Upon this principle, Dr. Porterfield's optometer was founded.

But, if the three points be supposed to be joined by a line, and this line to be somewhat inclined to the axis of the lens, each point of the line, except the first point (R, Fig. 6,) will have a double image; and each pair of images, being contiguous to those of the neighbouring radiant points, will form with them two continued lines, and the images being more widely separated as the point which they represent is further from the first radiant point, the lines (*s, s'; t, t'*;) will converge on each side towards (*r*) the image of this point, and there will intersect each other.

The same happens when we look at any object through two pin holes, within the limits of the pupil. If the object be at the point of perfect vision, the image on the retina will be single; but, in every other case, the image being double, we shall appear to see a double object: and, if we look at a line pointed nearly to the eye, it will appear as two lines, crossing each other in the point of perfect vision. For this purpose, the holes may be converted into slits, which render the images nearly as distinct, at the same time that they admit more light. The number may be increased from two to four, or more, whenever particular investigations render it necessary.

The optometer may be made of a slip of card-paper, or of ivory, about eight inches in length, and one in breadth, divided longitudinally by a black line, which must not be too strong. The end of the card must be cut as is shown in Plate XIV. Fig. 7, in order that it may be turned up, and fixed in an inclined position by means of the shoulders: or a detached piece, nearly of this form, may be applied to the optometer, as it is here engraved. A hole about half an inch square must be made in this part; and the sides so cut as to receive a slider of thick paper, with slits of different sizes, from a fortieth to a tenth of an inch

in breadth, divided by spaces somewhat broader; so that each observer may choose that which best suits the aperture of his pupil. In order to adapt the instrument to the use of presbyopic eyes, the other end must be furnished with a lens of four inches focal length; and a scale must be made near the line on each side of it, divided from one end into inches; and from the other according to the table here calculated from Cor. 7. Prop. IV, by means of which, not only diverging, but also parallel and converging rays from the lens are referred to their virtual focus. The instrument is easily applicable to the purpose of ascertaining the focal length of spectacles required for myopic or presbyopic eyes. Mr. Cary has been so good as to furnish me with the numbers and focal lengths of the glasses commonly made; and I have calculated the distances at which those numbers must be placed on the scale of the optometer, so that a presbyopic eye may be enabled to see at eight inches distance, by using the glasses of the focal length placed opposite to the nearest crossing of the lines; and a myopic eye with parallel rays, by using the glasses indicated by the number that stand opposite their furthest crossing. To facilitate the observation, I have also placed these numbers opposite that point which will be the nearest crossing to myopic eyes; but this, upon the arbitrary supposition of an equal capability of change of focus in every eye, which I must confess is often far from the truth. It cannot be expected, that every person, on the first trial, will fix precisely upon that power which best suits the defect of his sight. Few can bring their eyes at pleasure to the state of full action, or of perfect relaxation; and a power two or three degrees lower than that which is thus ascertained, will be found sufficient for ordinary purposes. I have also added to the second table, such numbers as will point out the spectacles necessary for a presbyopic eye, to see at twelve and at eighteen inches respectively: the middle series will perhaps be the most proper for placing the numbers on the scale. The optometer should be applied to each eye; and, at the time of observing, the opposite eye should not be shut, but the instrument should be screened from its view. The place of intersection may be accurately ascertained, by means of an index sliding along the scale.

The optometer is represented in Plate XIV. Fig. 8 and 9; and the manner in which the lines appear, in Fig. 10.

Table I. For extending the scale by a lens of 4 inches focus.

4	2.00	11	2.93	30	3.52	200	3.92	-35	4.51	-12	6.00
5	2.22	12	3.00	40	3.64	∞	4.00	-30	4.62	-11	6.29
6	2.40	13	3.06	50	3.70	-200	4.08	-25	4.76	-10	6.67
7	2.55	14	3.11	60	3.75	-100	4.17	-20	5.00	-9.5	6.90
8	2.67	15	3.16	70	3.78	-50	4.35	-15	5.45	-9.0	7.20
9	2.77	20	3.33	80	3.81	-45	4.39	-14	5.60	-8.5	7.56
10	2.86	25	3.45	100	3.85	-40	4.44	-13	5.78	-8.0	8.00

Table

Table II. For placing the numbers indicating the focal length of convex glasses.

Foc.	VIII.	XII.	XVIII.	Foc.	VIII.	XII.	XVIII.	Foc.	VIII.	XII.	XVIII.
0	8.00	12.00	18.00	20	13.33	30.00	180.00	8	∞	—24.00	—14.40
40	10.00	17.14	32.73	18	14.40	36.00	∞	7	—56.00	—16.80	—11.45
36	10.28	18.00	36.00	16	16.00	48.00	—144.00	6	—24.00	—12.00	—9.00
30	10.91	20.00	45.00	14	18.67	84.00	—63.00	5	—13.33	—8.57	—5.92
28	11.20	21.00	50.40	12	24.00	∞	—36.00	4.5	—10.29	—7.20	—6.00
26	11.56	22.29	58.50	11	29.33	—132.00	—28.29	4.0	—8.00	—6.00	—5.14
24	12.00	24.00	72.00	10	40.00	—60.00	—22.50	3.5	—6.22	—4.94	—4.34
22	12.77	26.40	99.00	9	72.00	—36.00	—18.00	3.0	—4.80	—4.00	—3.6

Table III. For concave glasses.

Number.	Focus and furthest place.	Nearest place.	Number.	Focus and furthest place.	Nearest place.	Number.	Focus and furthest place.	Nearest place.
0		4.00	7	8	2.67	14	3.00	1.71
1	24	3.43	8	7	2.54	15	2.75	1.63
2	18	3.27	9	6	2.40	16	2.50	1.54
3	16	3.20	10	5	2.22	17	2.25	1.44
4	12	3.00	11	4.5	2.12	18	2.00	1.33
5	10	2.86	12	4.0	2.00	19	1.75	1.22
6	9	2.77	13	3.5	1.87	20	1.50	1.02

VI. Being convinced of the advantage of making every observation with as little assistance as possible, I have endeavoured to confine most of my experiments to my own eyes; and I shall, in general, ground my calculations on the supposition of an eye nearly similar to my own. I shall therefore first endeavour to ascertain all its dimensions, and all its faculties.

For measuring the diameters, I fix a small key on each point of a pair of compasses; and I can venture to bring the rings into immediate contact with the sclerotica. The transverse diameter is externally 98 hundredths of an inch.

To find the axis, I turn the eye as much inwards as possible, and press one of the keys close to the sclerotica, at the external angle, till it arrives at the spot where the spectrum formed by its pressure coincides with the direction of the visual axis, and, looking in a glass, I bring the other key to the cornea. The optical axis of the eye, making allowance of three hundredths for the coats, is thus found to be 91 hundredths of an inch, from the external surface of the cornea to the retina. With an eye less prominent, this method might not have succeeded.

The vertical diameter, or rather chord, of the cornea, is 45 hundredths: its versed sine 11 hundredths. To ascertain the versed sine, I looked with the right eye at the image of the left, in a small speculum held close to the nose, while the left eye was so averted that the margin of the cornea appeared as a straight line, and compared the projection of the cornea with the image of a cancellated scale held in a proper direction behind the left eye, and close to the left temple. The horizontal chord of the cornea is nearly 49 hundredths.

Hence

Hence the radius of the cornea is 31 hundredths. It may be thought that I assign too great a convexity to the cornea; but I have corrected it by a number of concurrent observations, which will be enumerated hereafter.

The eye being directed towards its image, the projection of the margin of the sclerótica is 22 hundredths from the margin of the cornea, towards the external angle, and 27 towards the internal angle of the eye: so that the cornea has an eccentricity of one-fortieth of an inch, with respect to the section of the eye perpendicular to the visual axis.

The aperture of the pupil varies from .27 to .13 hundredths; at least this is its apparent size, which must be somewhat diminished, on account of the magnifying power of the cornea, perhaps to .25 and .12. When dilated, it is nearly as eccentric as the cornea; but, when most contracted, its centre coincides with the reflection of an image from an object held immediately before the eye; and this image very nearly with the centre of the whole apparent margin of the sclerótica: so that the cornea is perpendicularly intersected by the visual axis.

My eye, in a state of relaxation, collects to a focus on the retina, those rays which diverge vertically from an object at the distance of ten inches from the cornea; and the rays which diverge horizontally from an object of seven inches distance. For, if I hold the plane of the optometer vertically, the images of the line appear to cross at ten inches: if horizontally, at seven. The difference is expressed by a focal length of 23 inches. I have never experienced any inconvenience from this imperfection, nor did I ever discover it till I made these experiments; and I believe I can examine minute objects with as much accuracy as most of those whose eyes are differently formed. On mentioning it to Mr. Cary, he informed me, that he had frequently taken notice of a similar circumstance; that many persons were obliged to hold a concave glass obliquely, in order to see with distinctness, counterbalancing, by the inclination of the glass, the too great refractive power of the eye in the direction of that inclination, (Cor. 10. Prop. IV.) and finding but little assistance from spectacles of the same focal length. The difference is not in the cornea, for it exists when the effect of the cornea is removed by a method to be described hereafter. The cause is, without doubt, the obliquity of the uvea, and of the crystalline lens, which is nearly parallel to it, with respect to the visual axis: this obliquity will appear, from the dimensions already given, to be about 10 degrees. Without entering into a very accurate calculation, the difference observed is found (by the same corollary) to require an inclination of about 13 degrees; and the remaining three degrees may easily be added, by the greater obliquity of the posterior surface of the crystalline opposite the pupil. There would be no difficulty in fixing the glasses of spectacles, or the concave eye-glass of a telescope, in such a position as to remedy the defect.

In order to ascertain the focal distance of the lens, we must assign its probable distance from the cornea. Now the versed sine of the cornea being 11 hundredths, and the uvea being nearly flat, the anterior surface of the lens must probably be somewhat behind the chord of the cornea; but by a very inconsiderable distance, for the uvea has the substance

of a thin membrane, and the lens approaches very near to it: we will therefore call this distance 12 hundredths. The axis and proportions of the lens must be estimated by comparison with anatomical observations; since they affect, in a small degree, the determination of its focal distance. M. Petit found the axis almost always about two lines, or 18 hundredths of an inch. The radius of the anterior surface was in the greatest number 3 lines, but oftener more than less. We will suppose mine to be $3\frac{1}{4}$, or nearly $\frac{3}{10}$ of an inch. The radius of the posterior surface was most frequently $2\frac{1}{2}$ lines, or $\frac{2}{5}$ of an inch.* The optical centre will be therefore $\left(\frac{18 \times 30}{30 + 22} = \right)$ about one-tenth of an inch from the anterior surface: hence we have 22 hundredths, for the distance of the centre from the cornea. Now, taking 10 inches as the distance of the radiant point, the focus of the cornea will be 115 hundredths behind the centre of the lens. (Cor. 5. Prop. IV.) But the actual joint focus is $(91 - 22 =)$ 69 behind the centre: hence, disregarding the thickness of the lens, its principal focal distance is 173 hundredths. (Cor. 7. Prop. IV.) For its refractive power in the eye, we have (by Cor. 7. Prop. IV.) $n = 13.5$, and $m = 14.5$. Calculating upon this refractive power, with the consideration of the thickness also, we find that it requires a correction, and comes near to the ratio of 14 to 13 for the lines. It is well known that the refractive powers of the humours are equal to that of water; and, that the thickness of the cornea is too equable to produce any effect on the focal distance.

For determining the refractive power of the crystalline lens by a direct experiment, I made use of a method suggested to me by Dr. Wollaston. I found the refractive power of the centre of the recent human crystalline to that of water, as 21 to 20. The difference of this ratio from the ratio of 14 to 13, ascertained from calculation, is probably owing to two circumstances. The first is, that the substance of the lens being in some degree soluble in water, a portion of the aqueous fluid within its capsule penetrates after death, so as somewhat to lessen the density. When dry, the refractive power is little inferior to that of crown glass. The second circumstance is, the unequal density of the lens. The ratio of 14 to 13 is founded on the supposition of an equable density: but, the central part being the most dense, the whole acts as a lens of smaller dimensions; and it may be found by Prop. VII. that if the central portion of a sphere be supposed of uniform density, refracting as 21 to 20, to the distance of one-half of the radius, and the density of the external parts to decrease gradually, and at the surface to become equal to that of the surrounding medium, the sphere thus constituted, will be equal in focal length to a uniform sphere of the same size, with a refraction of 16 to 15 nearly. And the effect will be nearly the same, if the central portion be supposed to be smaller than this, but the density to be somewhat greater at the surface than that of the surrounding medium, or to vary more rapid externally than internally. On the whole, it is probable that the refractive power of the centre of the human

* Mem. de l'Acad. de Paris, 1730. p. 6. Ed. Amst.

crystalline, in its living state, is to that of water nearly as 18 to 17; that the water imbibed after death, reduces it to the ratio of 21 to 20; but that, on account of the unequable density of the lens, its effect in the eye is equivalent to a refraction of 14 to 13 for its whole size. Dr. Wollaston has ascertained the refraction out of air, into the centre of the recent crystalline of oxen and sheep, to be nearly as 143 to 100; into the centre of the crystalline of fish, and into the dried crystalline of sheep, as 152 to 100. Hence, the refraction of the crystalline of oxen in water, should be as 15 to 14: but the human crystalline, when recent, is decidedly less refractive.

(To be continued.)

V.

On the Measure of a Ship's Departure by the Reaction of the Fluid through which it passes.

By JOHN COOKE, Esq.

TO MR. NICHOLSON.

SIR,

July 22, 1801, No. 14, Kildare Street, Dublin.

SINCE the year 1793, when I first communicated my scheme to you for ascertaining distance at sea, by the ascent of water in a tube opposed to the stream, it occurred to me that departure might be measured in the same manner, and of several plans which I conceived for this purpose, the following seems to be the most practicable. It is an instrument similar to that described in the 48th page of the 4th Vol. of your Journal, with these alterations:

First, the horizontal and erect parts of the tube must be two distinct pieces, the former being moveable in an horizontal circle on the extremity of the latter, so that the erect part of the tube may be fixed to the vessel, and made water tight, without impeding the motion of the horizontal part of it.

Secondly, the rod which in the distance instrument is attached to the lee-way vane, is in this instrument attached to the bottom of the horizontal tube, so that the orifice of it may be pointed thereby in any horizontal direction, and screwed fast in that position; and the lee-way vane, which is unnecessary according to this method, is to be removed.

Thirdly, the extremity of the horizontal tube is to be so formed, as to be capable of moving up and down on horizontal pivots with two plates, like the fins of a fish extending behind it to keep the plane of the orifice in a vertical position, notwithstanding the heeling of the ship, which differs from the apparatus annexed to the distance instrument in this, that the object of the former is to present the orifice perpendicularly to the stream in all

cases, but in this it is only required to keep the orifice perpendicular to the horizon, though it be presented to the line of motion in an oblique direction.

If the horizontal tube of such an instrument be placed, by means of the directing rod and index, in a true east or west direction, according as the ship makes easting or westing, (which direction consequently must be altered as often as the ship's course is altered) the quantity of water discharged thereby will be proportionate to the departure, which appears from the following considerations:

When the plane of the orifice of this instrument coincides with the plane of a meridian, if the ship moves in a due east or west course, the direct impact of the water will occasion a discharge from the orifice, which measures the space passed over (as was shewn in the case of the distance instrument) but in sailing on a parallel circle this space is the departure.

Again, if the ship sails along a meridian with the orifice of the tube pointed as before, the stream will be parallel to the plane of the orifice, and therefore will occasion no pressure on it, or discharge of water from the aperture, in which case also the departure is represented by the result, because a ship sailing on a meridian makes no departure.

And lastly, if this instrument (the orifice being still in the plane of a meridian) be moved along a rhumb-line, the resistance which the lowest plate of water at the orifice meets, is the same as that which the orifice of a similar instrument would meet in passing over the departure of that rhumb in a direct position, and in the same time, because the pressure on each orifice is as the product of the force and number of impinging particles which strikes it; but the force of a single particle on the direct orifice, is to the force of a particle on the oblique orifice in a ratio composed of the ratio of their velocities, that is, as *departure to distance*, and of the ratio of radius to the sine of the angle of obliquity, that is, as *distance to departure*: therefore these forces are in a ratio compounded of the direct and inverse ratios of the same pair of quantities, which is the ratio of equality, and every particle strikes each orifice with equal force.

Again, the number of particles which strikes these orifices is the same, because they constitute two homogeneous columns, which having the same base, and lying between the same parallel planes are equal, whence it follows, that the pressures on these orifices are equal at all times, and consequently the quantities of water discharged through these apertures in equal times are equal also; but since the quantity discharged from the direct instrument would measure the space passed over by it, which is the departure, it is evident that the quantity discharged from the oblique instrument in the same time must give the departure also, and in general that the space given by such an instrument in an oblique position, is as the sine of the angle of obliquity; hence it is evident, that if this instrument be used with its orifice pointed to the north or south, instead of the east or west, that it will shew the difference of latitude, which furnishes the best method of trying the performance of the instrument; for if when used in this shape at sea, the difference of latitude

found.

found thereby should agree with that had from observation, it may be safely concluded, that it will measure departure with the same degree of certainty,

The principal advantage which the departure instrument seems to have over the distance instrument, is, that the error of lee-way is intirely avoided by it; this may be easily conceived, by considering that the magnetic needle points out the angle between the meridian and the line of the ship's keel, (supposing no variation) but since the line of her motion is seldom the line of her keel, this angle is seldom the true course; but in this method the line of the ship's keel does not enter into consideration, the angle made use of is that which is contained between the meridian, supposed to lie in the plane of the orifice and the direction of the stream, which must be the ship's true line of motion, and consequently this is the true course, without addition or deduction for lee-way.

If the moveable orifice at the extremity of the immersed tube were removed, it would simplify and improve the instrument much; the error which it is intended to correct, is that occasioned by the deviations of the plane of the orifice from perpendicularity, which when moving in an inclined position receives an undue degree of pressure, and misrepresents the departure; in this situation the pressure on the orifice is diminished as the square of the sine of inclination; therefore, if the spouting aperture were to be increased in area as the sine of inclination is diminished, the error would be exactly compensated, because the section of the spouting stream being then inversely as its velocity, the quantity of water delivered must remain the same, which may perhaps be effected in the following manner: let a sliding cover be joined to the spouting aperture, which may increase or diminish it by being moved up or down, and if the aperture be of such a form that its area may be enlarged in the ratio of the elevation of this slide, a pendulum moving steadily in the plane of the two parts of the tube, may be so connected with the slide as to give it a motion proportionate to the sine of inclination, which would have the desired effect; but the difficulty in this case would be to give the pendulum a slow deliberate motion: however, if its vibrations were to be regulated by the resistance of air contained in a compressible case, with small holes for its escape and return, all sudden vacillations would be checked thereby, and the pendulum would obey gradually every motion of the ship which might require this correction.

If you think that this Paper merits insertion in your Journal, it is at your disposal.

I am, SIR,

Your much obliged,

humble Servant,

JOHN COOKE.

VI.

*Report made to the Class of Physical and Mathematical Sciences of the Institute of France, by CITIZENS GUYTON and VAUQUELIN, on a Memoir of Citizen Thenard, entitled, "Observations on the Combination of the Tartareous Acid, with the Salifiable Bases, and the Properties of the Salts thus produced." **

IT has long been known that the tartareous acid has a strong tendency to form triple combinations; among which the salt of Seignette, martial tartar, tartar emetic, and various salts of this nature used in medicine are instances; but Citizen Thenard has greatly added to the number of these combinations, by others which have hitherto been unknown.

Triple Salts, Alcaline and Earthy.

The author was led to these discoveries by mixing solutions of lime, barites, and strontian, with the solution of the acidulous tartrate of pot-ash. He remarks, that the first portions of either of these earths produced no precipitation in the solution of tartrate of pot-ash, and that no change is seen in the fluid till the moment when the excess of the acid of the cream of tartar approaches its term of saturation; and that if this point be not exceeded, there will be formed on the sides of the vessel transparent crystals, in which analysis has shewn him the presence of tartareous acid, pot-ash, and the earth in question.

This first observation led him to presume, that the tartareous acid may be susceptible of many other combinations of this kind, and the experiments we are about to relate were the consequences.

1. A combination is formed between the tartrate of pot-ash and that of lime, from which results a salt with a double base soluble in water. This fact explains why no precipitate is afforded, when lime water is added to the tartrate of pot-ash, unless the quantity be such as to exceed what is necessary to form the triple salt.

This salt may be obtained in the crystalline form, if at the moment when the precipitation begins to manifest itself the fluid be left to repose. A short time afterwards it becomes clear, and deposits on the surface of the glass crystals, of which Citizen Thenard was not able to determine the figure.

2. Analogous combinations take place between the tartrate of pot-ash, barites, and strontian, each taken separately. This is shewn by the limpidity which the fluid preserves, so long as the mass of the earths added does not exceed certain limits. Now as the author remarks, this phenomenon cannot take place but by the effect of a triple combination, because the simple and neutral union of the tartareous acid with either of these earths, falls down as soon as it is formed.

3. The tartrate of magnesia has likewise a certain affinity with the tartrate of pot-ash, by virtue of which they contract an intimate union. The salt resulting does not crystallize;

it attracts the humidity of the air and becomes viscid by heat. Pot-ash separates the magnesia, which proves contrary to the opinion of Bergmann, that this alkali has a stronger attraction for the tartareous acid, than for that earth. It is more easily prepared by heating magnesia, recently precipitated from its solution, by a caustic alkali and still in the humid state, with the solution of cream of tartar.

4. From the union of alumen with the acidulous tartrite of pot-ash, a saline substance results which is not crystallizable, nor precipitated by the alkalies, whether pure or carbonated. To ascertain the cause of this singular effect, Citizen Thenard combined, by means of a gentle heat, gelatinous alumen to tartrite of pot-ash, and though a considerable quantity of the earth was dissolved, there was not any trace of pot-ash having separated.

This experiment shews the reason why pot ash does not occasion a deposition in the tartrite of alumen in combination with pot-ash, namely, that in this case the vegetable salt which is formed retains in solution the alumen separated from the tartareous acid.

The salt of Seignette presented the same properties.

5. He also mentions the ammoniacal tartrite of pot-ash, which crystallizes in fine transparent crystals, decomposable after a time by exposure to the air; this salt is already known.

Concerning the Alkaline, Earthy, and Metallic Acidulous Tartrites.

Citizen Thenard in this section shews, that the tartrites of soda, of magnesia, and of copper, unite with a super-abundance of their acid, and by that means produce acidulous salts, less soluble than the neutral tartrites. This property was before observed in the tartrites of pot-ash, of soda, and of ammonia.

If to a solution of tartrite of copper, which is very soluble, and difficultly crystallizable, there be poured a sufficient quantity of pure tartareous acid, a new combination is effected of sparing solubility, and of which almost the whole falls down under the form of a white blueish powder, which soda, pot-ash, and the carbonate of pot-ash dissolve completely without any separation of the copper.

The water in which the acidulous tartrite of copper has been precipitated, does not become blue by ammonia, which proves that no part of the salt has remained suspended, and consequently that it is of very sparing solubility.

Order of Attractions of Earthy and Alkaline Substances, with Regard to the Tartareous Acid.

After having composed the triple salts here spoken of, and exhibited their principal properties, Citizen Thenard determines by comparative experiments, the degrees of affinity which the alkalies and the earths have for the tartareous acid. According to him lime must occupy the first rank, and barites the second, and the others in the following order, strontian, pot-ash, soda, ammonia, magnesia, and alumen.

Metallic Alkaline Tartrites.

Citizen Thenard then passes to the examination of the metallic alkaline tartrites, among which he principally distinguishes those of manganese, zinc, tin, copper, lead, mercury,

and

and silver. In order to prepare them he makes use of different methods, according to the nature of the metal. Those which have the property of decomposing water, can be employed in the metallic state, others in the state of oxide, and some dissolved in the acids.

Zinc and iron are in the first class—copper, tin, and lead in the second—mercury and silver in the third.

The alkaline tartrites of manganese, of iron, of zinc, and of tin, are very soluble, and difficultly crystallizable. Their taste participates more or less of the metallic oxides which they contain. None of them is decomposed, or at least precipitated, either by caustic alkali, or the alkaline carbonates; but they are all decomposed by fulphurated hydrogen, hidro-sulphuret, and the gallic acid. There is however an exception with regard to the salt of manganese and fulphurated hydrogen.

The alkaline tartrite of copper, is distinguished from the other tartrites by its saccharine taste, and the great quantity of metal it contains. That of lead is not soluble. It is not decomposed by the alkalis, and what is remarkable, it is not decomposable even by the sulphates.

The tartrites of pot-ash and of silver, of pot-ash, and of mercury, do not follow the general rule; they are decomposed by the alkalies, the alkaline carbonates, fulphurated hydrogen, the hidro-sulphurates, the sulphates, and the muriates.

Though tartar emetic, or antimoniated tartarite of pot-ash, has long been a subject of inquiry by many chemists, and a number of interesting properties have been discovered in consequence, Citizen Thenard directed by new ideas, has made some additional discoveries. He shews for example, that a solution of this salt, which is decomposed and precipitated by the alkalies and the carbonates, ceases to be decomposed if a sufficient quantity of tartrite of pot-ash be added, in order that this salt may retain the oxide of antimony in solution.

It likewise entered into the plan of this chemist, to submit the tartar emetic to a more accurate analysis than has hitherto been made, and this analysis has required that of various other salts, particularly the nitrate of pot-ash or salt-petre, the proportions of the elements of which essentially interest many of the arts.

In order to obtain this result, he exposed 100 parts of crystallised transparent emetic tartar to a gentle heat, in order to drive off merely the water of crystallization. It was reduced by this operation to 92 parts. After having dissolved these 92 parts in water, he precipitated them by fulphurated hydrogen, which afforded 50 parts of golden sulphuret, in which there are, according to other experiments before made by him, 38 parts of oxide of antimony, such as it exists in the emetic tartar. In order to ascertain the quantity of tartareous acid, he mixed the acetite of lead with superabundance of acid, in the liquor separated from the golden sulphur, and he obtained one hundred parts of tartrite of lead, which contain, according to his analysis, 66 parts of oxide of lead, and 34 of tartareous acid.

Having

Having formed by these simple methods the proportions of water and tartareous acid, and of oxide of antimony in emetic tartar, there remained nothing more for Citizen Thenard to determine than the proportion of pot-ash, which would have been easy for him to have come at by subtracting the quantities already known from the sum total employed; but he chose rather to have recourse to experiment, and by that means to verify his former operations. He therefore took 100 new parts of emetic, calcined them in a crucible till the tartareous acid must have been decomposed, and afterwards treated the residue with weak nitric acid. He preferred this acid to all the others, because it does not dissolve the oxide of antimony, and the ley afforded him by evaporation, 30 parts of nitrate of pot-ash. He might have deduced the quantity of pot-ash contained in these 30 parts of nitre from the analysis given by many chemists, but the results of their analysis being very different, he could not know which to depend on. He therefore chose to resume this operation himself, and he found that this salt is formed of 53 parts of pot-ash, and 47 of nitric acid, and accordingly that 30 parts of nitre contained 16 parts of pot-ash. Whence we may establish the following proportions for 100 parts of emetic; namely, antimony 38, tartareous acid 34, pot-ash 16, water 8, and loss 4.

The acidulous tartrate of pot-ash being formed according to the analysis made by Citizen Thenard of 57 parts of tartareous acid, 33 of pot-ash, and 7 of water, and the tartareous acid dividing itself in the fabrication of tartar emetic, between the pot-ash and the oxide of antimony, we may conclude, that 100 parts of tartar emetic contained 34 parts of tartrate of pot-ash, 54 of tartrate of antimony, and 8 of water, because 100 parts of neutral tartrate of pot-ash contained 48 parts of acid, 43 of pot-ash, and 7 of water.

Citizen Thenard having obtained by analysis 57 parts of tartareous acid, 33 of pot-ash, and 7 of water from the acidulous tartrate of pot-ash; and having compared the proportions of the tartrate of pot-ash and antimony which constitute emetic tartar, he found that there is in cream of tartar more tartrate of pot-ash than is necessary to saturate the tartrate of antimony; that this excess of salt remains in the mother water of emetic tartar, and that in order to obtain this last salt in a state of purity, the crystals obtained from the first evaporation only must be collected.

CONCLUSION.

The experiments contained in the memoir, of which the Commissaries have given this account are numerous, interesting, and made as to them appears, with all the accuracy which chemical methods are susceptible of. They prove that most of the tartrates can combine with each other, and form triple salts which have particular properties. There are some in fact which have for their basis two alkalies, others an alkali and an earth, others an alkali and a metal, and others an earth and a metal. What is remarkable is, that most of these bases which are separated by the alkalis from their simple combinations with the tartareous acid, are not separated when in the state of triple salt.

Besides

Besides these interesting facts, this memoir contains other analyses which are very well made, of substances of consequence to several arts and to medicine, as to the accuracy of their proportions.

The commissaries are therefore of opinion that the class should order it to be printed in the volumes of the *Savans Etrangers*; and that Citizen Thenard would render a great service to chemistry by continuing to employ himself on the same plan upon these complex combinations of which different natural bodies are susceptible; this important object having been hitherto too much neglected by chemists.

VII.

On the Production of artificial Gold by Means of Muriate of Lime. By Mr. RICHARD WALKER. (Philos. Trans. 1801.)

(Concluded from page 227.)

PLATE XII. Fig. 1, represents the section of an apparatus, consisting of two vessels; viz. AA, is a vessel 4 inches in diameter, and 4 inches high, (omitting its stand, by which it rests over another vessel, presently to be described,) having a tube (of one piece with it) a , $\frac{7}{8}$ and $\frac{1}{2}$ in diameter, and 4 inches deep. This tube has a horizontal rim or shoulder, at $\frac{7}{8}$ of an inch from the top, * and is open at both ends; the lower one being closed occasionally, in the manner hereafter to be described; and the top of the tube, when the apparatus is of glass, by a stopple; or, if constructed of tin, by a sliding cover, fixed on the lid C.

C, is the cover or lid of this vessel, fitting over it quite close, and having a collar to fit over the tube, likewise, down to its shoulder.

B, represents a second vessel upon which the former fits closely, but not tight. This vessel is 3 inches and $\frac{6}{8}$ in height, having a conical cup (of one piece with it) b , 1 inch and $\frac{7}{8}$ diameter at the top, and 1 inch in diameter at the bottom, and three inches in depth: this cup is inclosed, at the distance of $\frac{1}{8}$ of an inch, by a thin partition c c , † of one piece likewise with the vessel.

* This vessel, when used, is to be filled up to this rim or shoulder *only*; that serving as a guard to prevent the frigorific mixture from getting into the tube: hence, the capacity of the vessel ending here, its height, and that of the tube, may be considered as 3 inches and $\frac{5}{8}$.

† Having ascertained by experiment, that a stratum of air of this thickness did not prevent (during the length of time which is required to freeze the water, and reduce the ice to powder in the tube a) the materials in the cup b from receiving the temperature required, yet was nevertheless sufficient to impede the action of the mixture on the materials, when mixed in the cup b , during the *short time* required to take its temperature, or to freeze the quicksilver, I adopted this method, in preference to letting out the frigorific mixture from the vessel B, immediately before mixing the last materials, as formerly.

D,

D, is the cover or lid of this vessel, fitting over it water-tight,* forming a bottom to the vessel, and having a rim $\frac{3}{8}$ of an inch deep, as a stand to insulate it from the table; the whole apparatus appearing to form, when together, one cylindrical vessel, 8 inches high, and 4 inches wide.† The vessels A and B contain each 1 pint and $\frac{1}{4}$; and the tube *a*, and cup *b*, 1 ounce and $\frac{1}{2}$ each.

N. B. The drawing, with the scale annexed, gives the section of this apparatus, of exactly the dimensions mentioned. The instrument described in Phil. Transf. for 1795, page 288, to be used with this apparatus, should be so long as just to pass through the bottom of the tube *a*, viz. $\frac{2}{8}$ of an inch below it.

Fig. 2, represents the *spirit thermometer* made use of in the experiments: it consists of three parts, viz. A is the thermometer, having its scale-board (made of box-wood) of a *semi-cylindrical* form, being flat in front, and round at the back, in order that it may be adapted to the cylindrical tube B B, in which it slides easily up and down, centrally; and may be occasionally taken out of it C, is a brass ferrule, cemented to, and forming one piece with, the tube, having a top or cover, which screws off and on. The scale extends from 100 below 0, to 100 above 0; the scale upwards being carried so far only as to allow of the unavoidable expansion to which the spirit may be subjected by atmospheric heat.

The thermometer (contrived by myself, and very accurately and neatly executed by Mr. Nairne, philosophical instrument-maker, in London,) is exactly *twice* the dimensions of the representation in the drawing, (as the scale,) and is graduated to single degrees; it has a common case, to make it portable.

N. B. A thermometer of the size described, is equally fit for an apparatus on a larger or smaller scale than that represented in Fig. 1.

The apparatus is used thus. The two vessels being taken apart, 1st. A circular piece of writing-paper is cemented ‡ over the bottom of the tube *a*. 2d. A frigorific mixture § is made

* If this cover does not fit water tight, it may be made so, by the intervention of a thin bladder previously soaked in warm water.

† For the purpose of ascertaining the proper proportion of the materials to be mixed at different temperatures, and other preparatory matters, I used an apparatus of the same construction as Fig. 3, Tab. XXIII. Phil. Transf. for 1795, but differing in having two cubes instead of one. The dimensions of this apparatus, being adapted to the same scale as the former, are thus: the vessel is $5\frac{1}{2}$ inches high, and $4\frac{1}{2}$ inches in diameter; the tubes are each $\frac{3}{8}$ of an inch in diameter, and 5 inches deep. The materials, being prepared separately in this vessel, were afterwards mixed in a wine-glass.

By means of this apparatus, in one instance, muriate of lime put into one tube, in a liquid state, and water in the other, were both consolidated by cold, then ground to powder, and afterwards mixed; but the salt did not grind well, and it moreover eroded and rusted the instrument.

‡ I use mucilage of gum arabic, or butter, if the other be not at hand; but it is sufficient merely to dip the paper in water, and apply it; the effect of the freezing mixture quickly cementing it.

§ A frigorific mixture, according to the intention, may be selected from the table in page 135 of this paper; or, in defect of ice, from the table in page 279 of Phil. Transf. for 1795. The mixture I use for

made in the vessel B, and the vessel covered by its lid D, then set upright, and four drams of muriate of lime put into the cup b. 3d. A similar frigorific mixture is made in the vessel A A, which is closed with its lid C. 4th. This vessel being placed over the other, as represented, three drams of water are to be poured gently, through a funnel, into the tube a, and the aperture closed. 5th. When the water is become perfectly solid ice,* the grinding instrument is to be put in, and, after suffering it to remain a short time to be cooled, the ice is to be ground gently to fine powder, (an assistant holding the apparatus firm,) and the instrument continued quite through the aperture at the bottom of the tube. 6th. The whole of the ice-powder is then to be forced into the cup of the lower vessel, and stirred about in it a little. 7th. The upper vessel being removed, and set aside, the muriate of lime and ice are to be thoroughly mixed; and a small tube, containing the quicksilver to be frozen, stirred about in the mixture; or the bulb of a spirit-thermometer, to take its temperature, which, if the experiment be conducted properly, will indicate, even if the experiment be made in summer, a cold of -50° . †

The apparatus, as represented in the drawing, is upon as fit a scale as may be required for common experiments; by attention, however, to the proportions given, one of any size may be procured.

this purpose, is that consisting of the solution of three different salts in water; and, in order to ascertain what proportions may be necessary to fill any sized vessel, I shall give the proportional quantities for a vessel containing in measure a *wine pint*, which are as follows: of muriate of ammonia 3 ounces, nitrate of potash 3 ounces, sulphate of soda 4 ounces and $\frac{1}{2}$, and water 10 ounces: having procured the salts separately in fine powder; I put these first into the vessel, and then fill up (without measuring) the vessel with water.

A mixture of this kind, made in the summer, when the temperature of the air is $+70^{\circ}$, will cool the materials to $+20^{\circ}$; and, if the salts and water are cooled to near $+50^{\circ}$, previously to mixing, by immersion in cold water, to $+10^{\circ}$. My usual method is, (without taking the usual precaution of cooling the salts,) to add the water much reduced in temperature, by pumping off a bucket or more first, by which the materials, consisting of muriate of lime and ice, are cooled to $+15^{\circ}$ before mixing.

In winter, the experiment may be conducted by adding snow, at the temperature of the air, to muriate of lime, (cooled to a low temperature,) in the vessel B. In summer, by adding ice-powder, cooled to a low temperature, in the vessel A, to muriate of lime cooled to $+50^{\circ}$, by water, (instead of a frigorific mixture,) in the vessel B.

The temperature of the springs, or of well-water, it is well known, is in this climate nearly $+50^{\circ}$ throughout the year.

* At this period, I shake the apparatus, in order to expedite the solution of the salts, and to diffuse the effects of the freezing mixtures; or, if necessary, *renew* them.

† Muriate of lime and ice-powder, mixed at the temperature of $+20^{\circ}$, give a cold of -48° ; if mixed at $+15$, of -51° ; and, at $+10^{\circ}$, a cold of -54° .

The freezing point of quicksilver is -39° ; but that metal requires a temperature of -45° , to assume its perfectly solid state.

I have repeatedly frozen quicksilver in the middle of summer, by mixing together muriate of lime and ice-powder, at $+20^{\circ}$; and likewise, by mixing together nitrous acid and ice-powder, at $+8^{\circ}$.

Muriate

Muriate of lime produces no effect upon tin or japanned vessels; hence the apparatus is best made of common block-tin; observing that the tube and cup be made of the *thinnest* tinned iron, and the whole besides of the same substance, but considerably *thicker*. *

The tube and cup should be very smooth within-side, and perfectly central; the tube having as little seam as possible, that the grinding instrument be not obstructed.

The grinding instrument acts best when the edge, instead of being quite horizontal, is a little inclined from each shoulder, towards the centre.

In order to keep this paper within tolerable limits, I have carefully avoided a repetition of all matters mentioned in my former papers on this subject; I must therefore refer to those, especially that "On the best Methods of producing artificial Cold," † for the particular mode of conducting experiments on cold; this being essentially the same in principle, whatever be the materials made use of to effect it. Hence, the apparatus just described is applicable to the use of the *mineral acids*, ‡ as well as to that of *muriate of lime*; recollecting that it is necessary to substitute glass for tin, when the former are used; or to give the inside of the cup, or vessel containing it, a coating of wax, to defend the tin from their action.

Having given an account of Pr. Lowit's experiments on the power of muriate of lime for producing artificial cold, and added such observations of my own as resulted from them, I shall conclude by exhibiting a general view of the different frigorific mixtures composed of chemical substances with *ice*, as I have before done, (Phil. Trans. for 1795, page 279,) of those in which the use of ice is dispensed with.

* The best method of constructing vessels for the purpose of excluding heat, is obviously to have them made of the best non-conducting substance, lined within with the best conducting substance; hence these vessels (the tube excepted) might be fitter for the purpose, if made of wood lined with tin.

My general rule for constructing the apparatus, is to allow *four* times, by measure, the water to be frozen and reduced to powder, in the capacity of the tube that is to contain it; and *three* times the weight (by measure) of the muriate of lime, to the cup in which the muriate of lime is to be cooled, and the ice-powder afterwards added to it: and, when nitrous acid is used, (instead of muriate of lime,) *four* times its weight; and about four times the diameter of the vessels to that of the tube.

The preparatory mixtures, that is, such as are used for cooling the materials previously to mixing, are best made of *ice* and salts; these retaining their temperature longer than those consisting of solutions of salts in water or acid; but, in either case, if necessary, they may be occasionally renewed, after the water is become solid.

† Phil. Trans. for 1795, p. 270.

‡ Nitrous acid, and vitriolic acid, may at any time be immediately procured from a chemist's shop; whereas the muriate of lime, not being used for any other purpose, must commonly be prepared for this *alone*; hence it may not unfrequently happen that the former, on this account, may be preferred.

CLASS I.

Acids and salts.	Ice.	Temp. of mat. before mixing	Temp. or cold. produced
Muriate of soda 1, - - - - -	Snow 2, - - -	- - -	- 5°
2, Muriate of ammonia 1, - - - - -	5, - - -	- - -	- 12°
10, - - - - - 5, Nitrate of pot-ash 5, - - - - -	4, - - -	- - -	- 18°
5, Nitrate of ammonia 5, - - - - -	12, - - -	- - -	- 25°

CLASS II.

Diluted vitriolic acid 2,* - - - - -	Snow † 3, - - -	+ 32°	- 23°
Concentrated muriatic acid 5, - - - - -	8, - - -	+ 32°	- 27°
Concentrated nitrous acid 4, ‡ - - - - -	7, - - -	+ 32°	- 30°
Muriate of lime 5, § - - - - -	4, - - -	+ 32°	- 40°
3, ** - - - - -	2, - - -	+ 32°	- 50°
Caustic vegetable alkali 4, - - - - -	3, - - -	+ 32°	- 51°

The above table is divided into two classes. The first class consists of mixtures of salts and ice, in which the temperature of mixing is of no consequence, the effect produced being the same at any temperature of the air: the salts should be in the state of powder. Ice pounded small may be substituted, with equal effect, for snow.

The second class consists of such mixtures as will produce an effect *greater*, the colder the temperature is at which the materials are mixed, but in a *diminishing* ratio; ceasing entirely at that degree of cold at which the composition itself freezes. †† The salts should be in the state of fine powder.

N. B. The figures after the salts, or acids, and ice, expresses the proportions, *by weight*, to be used.

• Concentrated vitriolic acid, diluted with half its weight of snow, or distilled water, and cooled:

† Snow that is fresh, dry, and uncompressed, or such as has never been subject to the effects of a temperature less than freezing; or, when such is not to be procured, ice reduced to powder, in the manner described in Phil. Trans. for 1795, p. 271, may be substituted in its stead, with equal effect.

‡ Concentrated *fuming* nitrous acid alone; or concentrated *pale* nitrous acid, diluted with one-fifth its weight of snow, or distilled water, and cooled.

§ Of the strength of 1,490, at 80° of heat.

** Of the strength of 1,450, at 80° of heat.

†† The materials may be cooled, previously to mixing, when required, by a frigorific mixture taken from the table: for this purpose, either of the mixtures in Class I. are convenient; particularly the first, consisting of muriate of soda and snow.

In the above table, the ordinary effect of snow, or ice-powder, is given; but, if the latter be prepared (ground) with a sharp instrument, using light pressure, the effect will be somewhat greater, the ice being then reduced to an impalpable powder: hence, ice powder, thus obtained from a block of ice, may at any time be substituted for snow.

Cold is produced by mixing various other chemical substances with ice: in the above table, such only are retained as produce that effect in a remarkable degree.

As the new nomenclature is now generally adopted, I have used it in this paper.

POSTSCRIPT.

In the course of my former papers on the subject of cold, I have had occasion to make, incidentally, some remarks on the power of water, under certain circumstances, in resisting an extraordinary degree of cold without freezing; likewise on the particular kind of agitation which induces water, cooled below its freezing point, to crystallize or become ice.† As these are subjects which have likewise engaged the attention of others, I shall take the liberty of barely mentioning a fact, having relation to those points, which has lately occurred to me.

It is a remarkable circumstance respecting artificial freezing, that the ice thus procured in the usual way, (viz. by immersing the water to be frozen, in a convenient vessel, in a frigorific mixture,) will always be more or less opaque, never transparent: this I had constantly remarked, without much attending to it; however, having in the course of last summer been induced to try the effect of an ice-speculum for producing heat, it became necessary that the ice, which in this instance was substituted for glass, should be perfectly transparent. After varying the process in every possible way I could think of, by *immersing the water* to be frozen, without effect, I at last succeeded completely, by forming a coating of ice, of sufficient thickness, on the outside of a vessel containing the frigorific mixture; the bottom of this vessel, which was made concave for this particular purpose, being immersed for a sufficient length of time in a shallow pan of water.

Hence arises the means, before unknown to me, of obtaining ice, either in an opaque, or perfectly transparent state; moreover, water, as I have experienced lately, constantly forms a coating of ice on the outside of a vessel containing a frigorific mixture, so soon as it is cooled to $+32^{\circ}$.

* Phil. Transf. for 1788, page 401.

SCIENTIFIC NEWS; ACCOUNTS OF BOOKS, &c.

Philosophical Transactions of the Royal Society of London for the Year 1801. Part the First. Quarta, 240 Pages, with fifteen Plates, and a Meteorological Journal. London, sold by Elmsly.

THIS part contains;—1. The Croonian Lecture. On the Irritability of Nerves, by Everard Home, Esq. F. R. S. 2. The Bakerian Lecture. On the Mechanism of the Eye, by Thomas Young, M. D. F. R. S. (Philos. Journal V. 253.) 3. On the necessary Truth of certain Conclusions obtained by means of imaginary Quantities. By Robert Woodhouse, A. M. Fellow of Caius College. 4. On the Production of artificial Cold, by means of Muriate of Lime. By Mr. Richard Walker. (See Phil. Journal V. 222, 272.) 5. Account of a monstrous Lamb, by Mr. Anthony Carlisle. 6. An Anatomical Description of a male Rhinoceros. By Mr. H. Leigh Thomas, Surgeon. 7. Demonstration of a Theorem, by which such Portions of the Solidity of a Sphere are assigned as admit an Algebraic Expression. By Robert Woodhouse, A. M. Fellow of Caius College, Cambridge. 8. Account of the Discovery of Silver in Herland Copper Mine. By the Rev. Malachi Hitchins. 9. Account of an Elephant's Tusk, in which the Iron Head of a Spear was found imbedded. By Mr. Charles Combe. 10. Description of the Arseniates of Copper and of Iron from the County of Cornwall. By the Count de Bournon. 11. Analysis of the Arseniates of Copper and of Iron, described in the preceding Paper; likewise an Analysis of the red octahedral Copper Ore of Cornwall; with Remarks on some particular Modes of Analysis. By Richard Cheneyix, Esq. M. R. I. A. Appendix. Meteorological Journal kept at the Apartments of the Royal Society, by order of the President and Council.

NATIONAL INSTITUTE OF FRANCE.

Extract from the Notice of the Works of the Class of Mathematical and Physical Sciences, of the National Institute of France during the third Trimestre of the Ninth Year.

Natural Philosophy.—New Galvanic Experiments.—The combustion of metals by this power as related at page 239 of our last Number, was discovered by Fourcroy, Vauquelin, and Thenard, and not by Fourcroy alone as there stated.

Chemistry.—On the Reduction of the Oxides of Zinc by Charcoal.—The newly discovered combination of oxygen with carbon in the form of an oxide, upon which Mr. Cruickshank has published such an excellent course of experiments in this Journal, has likewise been the subject of attention with Dr. Woodhouse of America, and the French chemists Deformes, Berthollet, Clement, Guyton, Fourcroy, Thenard, Hassenfratz. As this sub-

ject will form the basis of more ample communication and research, I shall not here attempt to give any more concise abridgement of the communications to the Institute.

Mineralogy.—Concerning arseniated Copper in Plates.—This mineral from Cornwall, mentioned by some German chemists, but little known in France, has been described by Delievre, and analysed by Vauquelin. It crystallizes in brilliant hexagonal plates, transparent, olive-green, which decrepitate in the flame of a candle, to which they give a green colour: it is very difficultly fusible by the blow-pipe, emits a smell of garlic, colours borax green with reddish zones, and is soluble with efflorescence in the nitric acid, to which it gives a slight green colour.

Geology.—On the Eruption of Mount Vesuvius, in the second Republican Year.—One of the most important points to be determined in the history of volcanos, is the degree of heat necessary to give fluidity to the lavas. Is this fire similar in intensity to that which produces glass? or is the fluidity owing to some other cause? Dolomieu has been long employed on this question. The eruption of Vesuvius in the second year afforded him the means of ascertaining this degree of heat almost thermometrically. He found that this heat is not greater than fuses silver, but less than is required to fuse copper. The metals capable of being oxidized at a less heat than that which is required to fuse them, have been oxidized in the centre of the most voluminous masses. Lead was converted into tessular galena with large scales, glass into Reaumur's porcelain, &c.

On the Belemnites.—Citizen Sage, who described several new species of this fossil in the *Journal de Physique*, communicates in this article some others, which at that time he had not seen.

Meteorology.—Agreement between the Variations of the Atmosphere over a great Extent of Country.—The labours of Citizen Lamark, to determine whether the variations of the atmosphere be governed by any periodical law are well known. On the present occasion he has endeavoured to ascertain whether these variations extend to great distances. For without this condition, every method of predicting the atmospheric changes must undoubtedly be merely local; and different rules would be required, even for the greater variations, at each different place. His present inquiries are confined to the barometrical changes. He traced twenty-six parallel lines upon paper, to represent the stations or heights at which the barometrical column stands in our climate. Other perpendicular lines represent the days in succession. By this ingenious contrivance (which was adopted very early in the last century) he was able to exhibit a curve denoting the barometrical variations for any given course of time. But the inquiries of Lamark demanded, that he should trace in the same picture the lines for different places. One of these answers to the observatory at Paris; a second to the residence of Citizen Picot of Geneva; a third to that of C. Thulis at Marseilles; and he found that these lines ascend and descend generally together, and differ scarcely at all, but according to the height of the place of observation, or the extent of the variations, but not their direction.

Zoology.—

Zoology.—On the monocular Flea.—By the vulgar name of water-flea is known a small crustaceous animal, very abundant in stagnant waters, which has sometimes given rise to the story of showers of blood, because in the spring its ova with which it is filled, give it a red colour, and the waters containing many of these animals do then in fact appear as if they contained blood. The most skillful naturalists, Swammerdam, De Geer, Schœffer, and Otto-Frederick Muller, have successively studied this animal, but nature is inexhaustible in her smallest productions. Citizen Jurine, associate of the Institute of Geneva, has discovered in this insect a number of curious circumstances which had escaped those learned men. He has described at large its internal and external organization, its sexual intercourse, &c. The most singular fact he has discovered is, that the female, which has received the male, transmits the influence to its female descendants, so that they all produce eggs, without recourse to the male to the sixth generation, after which their young perish in the hatching. Another species has carried this influence of a single copulation to the fifteenth generation. It is known that fleas have afforded similar observations to Bennet. These generations without conjunction are less abundant, and succeed each other less rapidly than those in which the communication of the male has taken place.

Botany.—Description of the new, or scarcely known plants in the garden of Citizen Cels, by C. Ventenat. Of the magnificent work here described, four livraisons have already appeared, each containing ten plants.

History of the Oaks of America.—It is somewhat singular, that the species of the oak which are so few in Europe, should be so diversified under the same latitude in America. C. Michaux has given a history of these numerous species, which is the more interesting, as it is probable that some of them may be of much utility when transplanted into Europe.

Anatomy.—On the Symphysis of the Pubis.—Citizen Tenon, who has long been occupied on this part of the anatomy of the human subject, has ascertained that the os pubis does not separate but 'in such pulvis' as have a double cartilage, in which case the ligaments only are lengthened, and that this structure is the least frequent.

Technology.—New method of washing linen. For the account of this process of Chaptal, vide our last Number, page 233.

Paper making.—C. Seguin, who has been five years employed on the art of manufacturing paper, has made several useful discoveries to expedite the progress. The manufacture of paper from straw discovered so long ago in Germany, and lately carried into effect in this country, is among the improvements of C. Seguin. But he has not communicated any of his methods to the Institute.

Fig. 1.

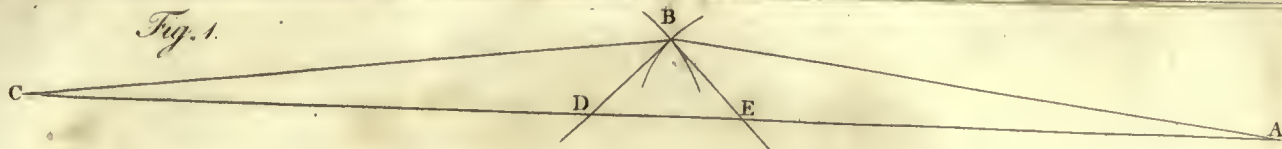


Fig. 2.

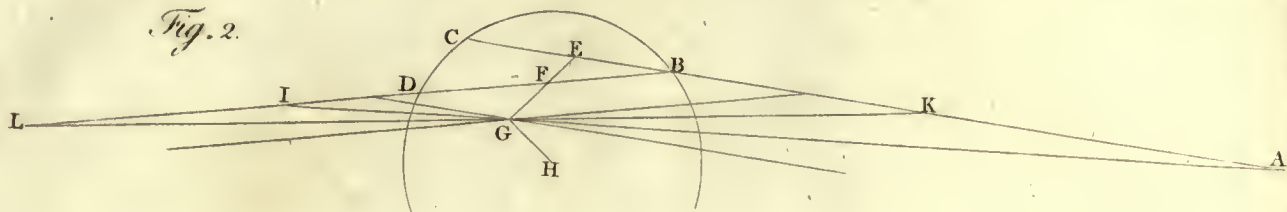


Fig. 3.

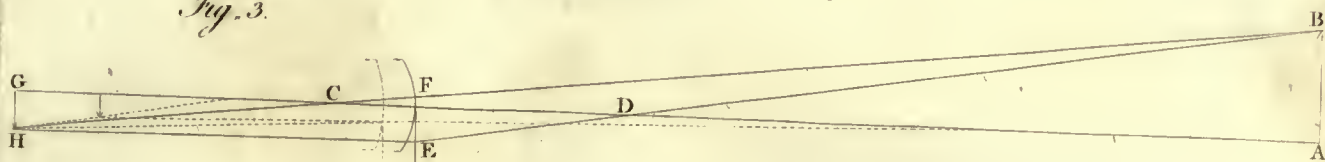


Fig. 4.

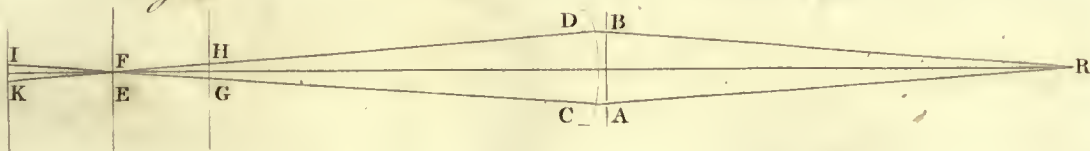


Fig. 5.

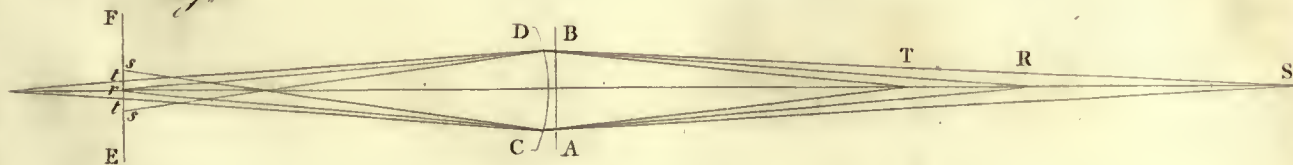


Fig. 6.

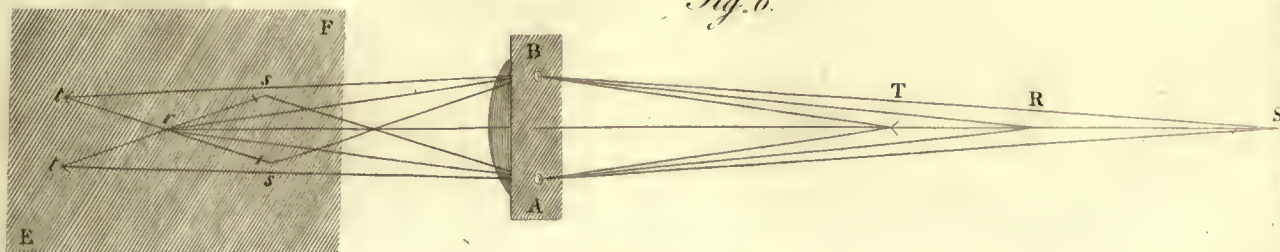




Fig. 8.

SCALE OF INCHES.	8	7
FOCUS OF CONVEX, NEAREST X.	1	1
	2	2
	3	3
	4	4
	5	5
	6	6
	7	7
	8	8
	9	9
	10	10
	11	11
	12	12
	13	13
	14	14
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ARTICLE I.

*Outlines of Observations relating to Nitrous Oxide, or Dephlogisticated Nitrous Air. By Mr. DAVY, Director of the Chemical Laboratory, and Lecturer on Chemistry in the Royal Institution.**

I.

SECT. I. The æriform fluid, that constitutes the subject of the following observations, was called by its discoverer, Dr. Priestley, dephlogisticated nitrous air. The Dutch chemists, who, some time ago, investigated certain of its properties, named it gaseous oxide of azote. Lately, in this country, it has been denominated nitrous oxide, which, according to the principles of the French Nomenclature, signifies nitrogen in the first degree of oxidation. This term cannot well be confounded with nitrous gas;† and it bears the necessary analogy to words denoting other combinations of oxygen and nitrogen.

* Extracted from "Researches, Chemical and Philosophical, concerning Nitrous Oxide, 1800, Johnson," with additions, and communicated by the author.

† Nitrous gas, according to the *first* principles of the French Nomenclature, ought to be called *nitric oxide*.

Sect. 2. A short and general account of the production and properties of nitrous oxide will not, perhaps, be wholly unacceptable to the chemical world; particularly as the most remarkable agencies of this substance are but little known out of Great Britain; and they are connected with curious physiological enquiries.

II. On the Production of Nitrous Oxide.

Sect. 1. As yet nitrous oxide has not been composed immediately from its elements; and there is no reason to believe that it is produced in any of the operations of nature. But it may be procured in a number of artificial processes, during the decomposition of nitrous gas and nitric acid.

Sect. 2. Nitrous gas may be converted into nitrous oxide in two modes. First, by the simple abstraction of a portion of its oxygen by bodies possessing a strong affinity for that principle, such as alkaline sulphites, muriate of tin, and sulphurets. Secondly, by the combination of a body with portions both of its oxygen and nitrogen, such as hydrogen, when in a peculiar state of combination, or in a nascent form.

a. The alkaline sulphites convert nitrous gas into nitrous oxide with much greater rapidity than any other substances.

At temperature 46°, Fahrenheit, 16 cubic inches of nitrous gas were converted in less than an hour into 7,8 cubic inches of nitrous oxide, by 100 grains of pulverized sulphite of pot-ash containing its water of crystallization. During this process, the sulphite became partially converted into sulphate; but no *sensible* increase of temperature was produced. No water was decomposed, nor was any free nitrogen evolved.

Muriate of tin, and the sulphurets,* whether solid or in aqueous solution, act slowly upon nitrous gas, and become oxygenated, whilst they convert it into nitrous oxide. The diminution of the nitrous gas, in these processes, is the same as in the process with sulphite of pot-ash.

b. When nitrous gas and sulphurated hydrogen gas are mingled together, they slowly decompose each other, with great diminution of volume. In this operation, nitrous oxide is formed, † sulphur is deposited, and signs of the production of ammoniac and water are perceived. There is every reason to believe that the hydrogen of the sulphurated hydrogen is the great agent in effecting this decomposition, by combining with portions of both the oxygen and nitrogen of the nitrous gas, to produce water and ammoniac. For no perceptible quantity of acid is formed in the experiment; and the sulphur deposited appears to be in its common state. This opinion indeed is further proved, by the phenomena connected with the decomposition of nitrous gas by nascent hydrogen.

* Dr. Priestley first noticed the conversion of nitrous gas into nitrous oxide by sulphurets. To the Dutch chemists we owe the discovery of the action of muriate of tin on nitrous gas. *Journal de Physique*, XLIII. part II. page 326.

† Mr. Kiwan first observed the peculiar action of sulphurated hydrogen on nitrous gas. *Phil. Trans.* Vol. LXXVI. page 133.

When nitrous gas is exposed to nascent hydrogen, whether it be procured by the action of metals upon water, in common cases, or in Galvanic experiments, ammoniac and nitrous oxide are uniformly produced; and the volume of the nitrous gas diminishes nearly to the same extent, as in the process with sulphurated hydrogen, *i. e.* to about one-third.

SECT. 3. Nitrous oxide may be procured from nitric acid, when this substance is decomposed by certain metallic bodies; and likewise during the mutual decomposition of nitric acid and ammoniac at certain temperatures.

a. During the action of zinc, iron, or tin, upon diluted nitric acid, certain quantities of nitrous oxide are produced, but mingled with nitrous gas and nitrogen. In these processes, as has been long known, ammoniac is formed; and consequently water is decomposed: a circumstance most probably connected with the production of the nitrous oxide; for the quantity of the gas is less in proportion as the acid is more concentrated.

b. Nitrous oxide,* perfectly pure, may be obtained in an easy manner, and at a cheap rate, from the decomposition of nitrate of ammoniac. This salt, when deprived of as much of its water of crystallization as possible, becomes fluid at the temperature of 320° , Fahrenheit, and immediately begins to be converted into water and nitrous oxide.*

One hundred grains of salt, perfectly decomposed at any temperature between 320° and 620° , produces about 57 grains of gas, and 43 of fluid. In experiments on this decomposition the heat must be carefully applied and regulated; as at temperatures above 800° , nitrate of ammoniac detonates, and becomes changed into nitrous gas, nitric acid, water, and nitrogen.

III. On the Combinations of Nitrous Oxide.

SECT. 1. Pure water is capable of holding in solution, at the common temperatures of the atmosphere, a quantity of nitrous oxide equal to about half its volume.† Water, impregnated with nitrous oxide, has a sweetish taste, which is hardly perceptible, except to delicate organs: and it becomes effervescent on agitation. It is possessed of no action in vegetable colours; and effects no perceptible change in metallic solutions.

Nitrous oxide is expelled unaltered from water at temperatures equal to about 212° . It is likewise incapable of remaining in combination with it, when the pressure of the atmosphere is removed.

Nitrous oxide is expelled from water by muriatic acid, carbonic acid, and sulphurated hydrogen; but it has a stronger affinity for that fluid than nitrous gas, oxygen, or hydrogen.

* To Mr. Berthollet we owe the discovery of the products evolved during the slow decomposition of nitrate of ammoniac. See *Mem. de Paris*, 1785.

† Dr. Priestley's Experiments and Observations, Vol. II. page 81.

Se \AA t. 2. Ether, alcohol, and the volatile and fixed oils, dissolve nitrous oxide in larger quantities than water at common temperatures; but without undergoing any material alteration in their properties. The gas is incapable of remaining in combination with them at the temperature of their ebullition, or under the exhausted receiver.

Se \AA t. 3. Nitrous oxide has as yet been combined with no solid bodies, except the fixed alkalis; and even the affinities of water, alcohol, ether, &c. for this gas appear to be diminished when they hold in solution neutral salts and other substances. The sulphurets, the sulphites, and the different bodies that act upon nitrous gas, both dry and in solution, effect no alteration in the composition of nitrous oxide.

Se \AA t. 4. The maria tjc, and the sulphureous acid gases, expand a little when mingled with nitrous oxide; but they are readily separated from it by water. No other aëriiform fluids appear to be possessed of the least power of action upon this gas at common temperatures.

Se \AA t. 5. None of the alkaline bodies, except pot-ash and soda, have as yet been combined with nitrous oxide; and even these substances are capable of uniting with it, only when it is in the nascent state.

a. When nitrous oxide in a free state is exposed to the dry alkalis, or alkaline earths, at common temperatures, it is neither absorbed nor acted upon. When it is placed in contact with solutions of them in water, a small quantity of it is dissolved: this phenomenon, however, appears to depend, in a great measure, on the water of the solution; for the gas may be expelled from it unaltered at the temperature of its ebullition.

b. To form the combination of pot-ash, or soda, and nitrous oxide, the alkalis, after being mingled in the dry state with alkaline sulphites, must be exposed, together with them, to nitrous gas. The nitrous oxide, as fast as it is formed by the action of the sulphite on the nitrous gas, is absorbed by the free alkali; and thus a permanent union is effected between the two bodies.

c. The compound of nitrous oxide and pot-ash is very soluble in water, but apparently insoluble in alcohol. Its taste is not very different from that of caustic pot-ash, perhaps a little more pungent. When acted upon by the acids it gives out nitrous oxide. This gas is likewise disengaged from it in a pure form by the application of a heat above 400°, Fahrenheit. It produces scintillations when thrown upon red hot charcoal.

d. The properties of the combination of soda and nitrous oxide are not materially different from those of the compound of pot-ash and nitrous oxide.

IV. *Of the Action of Heat and Electricity on Nitrous Oxide.*

Se \AA t. 1. Nitrous oxide undergoes no change in its composition at temperatures below those of ignition, though exposed to them for ever so great a length of time. When, however, it is passed through a porcelain tube, heated red, or when it is acted on for a long while by the electric spark, a new arrangement of its principles takes place, and it becomes.

becomes converted into nitrous acid, and a gas analogous to atmospheric air, but containing a little less oxygen. *

The nitrous acid formed in this process is very small in quantity, but the unabforbable gas equals about $\frac{1}{8}$ of the original volume of the nitrous oxide.

V. *On the Decomposition of Nitrous Oxide by Combustible Bodies.*

SECT. 1. The changes produced in nitrous oxide by heat, are probably intimately connected with the phenomena of its decomposition by the inflammable bodies, which in general require, for their combustion in this gas, much higher temperatures than those at which they burn in atmospheric air or oxygen. When they are intensely heated, they act upon it, and become oxygenated, nitrogen being evolved, and much heat and light generated.

During the combustion of solid and fluid bodies producing flame in nitrous oxide, nitrous acid is formed, most likely in consequence of a new arrangement of principles produced by the ignition of a part of the gas not in contact with the burning substance. Likewise when nitrous oxide in excess is decomposed by inflammable gases, nitrous acid, nitrogen, and a little oxygen, are produced, probably from the same cause.

a. Pyrophorus, which inflames in nitrous gas and in atmospheric air, at or even below 40° , Fahrenheit, requires for its combustion in nitrous oxide a temperature nearly equal to that of ignition.

b. Phosphorus may be fused and even sublimed in nitrous oxide without effecting any change in it; but when it is introduced into it in a state of active inflammation, it burns with intensely vivid light.

c. Sulphur does not act in nitrous oxide when introduced into it, burning with the feeble blue flame; but if in a state of vivid inflammation, it decomposes it with the production of a beautiful rose-coloured light.

d. Iron and charcoal, when heated white, burn in nitrous oxide with much greater vividness than in the atmosphere.

e. The lighted taper, introduced into nitrous oxide, burns at first as in oxygen; but afterwards with a flame, white in the centre, and blue at the circumference; a phenomenon apparently depending upon the nitrous acid formed in the process.

f. Hydrogen, and the compound inflammable gases, when mingled in certain proportions with nitrous oxide, and acted on by the electric spark, explode, with the production of much light and heat. †

* Dr. Priestley first observed, that nitrous oxide, after being acted upon by the electric spark, became immiscible with water; but he did not notice the nitrous acid produced in the process: neither did the Dutch chemists, who repeated his experiments.

† The Society of Chemists at Amsterdam thought that hydrogen was the only body capable of decomposing nitrous oxide. *Journal de Physique*, tom. XLIII. part II. page 331. From their experiments it appears probable that in many cases they did not raise the temperatures of other inflammable bodies sufficiently high for this purpose.

Sect. 2. When nitrous acid is formed in processes of combustion in nitrous oxide, so as to remain in the residual gas, the quantity of free nitrogen evolved never equals more than $\frac{2}{3}$ of the volume of the nitrous oxide decomposed, though when this substance is not produced, 10 of nitrous oxide give out about 11 of nitrogen.

Sect. 3. The heat produced by even the slow combustion of bodies in nitrous oxide, is in all cases sufficient to keep up the process for a certain time, after the original heating cause is removed: but in proportion as the volume of nitrogen increases, the inflammation diminishes in vividness, and it uniformly ceases before the whole of the gas is decomposed. Charcoal and iron cease to burn in nitrous oxide when it is mingled with an equal volume of nitrogen; yet the combustion of phosphorus will continue (though feebly) in a gas consisting of 4 nitrogen and 1 nitrous oxide.

VI. *On the Analysis of Nitrous Oxide.*

Sect. 1. *a.* One hundred cubic inches of nitrous oxide weigh at temperature 55°, Fahrenheit, and atmospheric pressure 30. about 50.2 grains.

b. Four cubic inches of this gas, when perfectly decomposed by the combustion of charcoal, are converted into about 2.2 cubic inches of carbonic acid, and 4.2 cubic inches of nitrogen; no nitrous acid being produced in the process.

c. Likewise 2 cubic inches of nitrous oxide require, for their complete decomposition, about 2.2 cubic inches of hydrogen, and evolve during the inflammation, a quantity of nitrogen, nearly equal to it in volume.

d. Comparing these facts with other facts relating to the production of nitrous oxide, we may conclude that 100 parts of this gas are composed of about 37 parts oxygen and 63 parts nitrogen, * existing in a much more condensed state than when in their simple forms.

e. The oxygen appears to be combined with the nitrogen much more intimately in nitrous oxide than in nitrous gas; a phenomenon that may be easily accounted for, when we consider the general law of saturation. †

VII. *On the Action of Nitrous Oxide on Living Beings.*

Sect. 1. Nitrous oxide when taken into the mouth tastes faintly sweetish. Its odour is weak, but agreeable. It is respirable, and produces very peculiar effects, when made to act on the system by means of the lungs.

The sensations it occasions are in general analogous to those connected with intoxication from fermented liquors.

* This estimation is very analogous to that of the Dutch chemists. *Journal de Physique*, tom. XLIII. part II. page 331.

† The affinity of one body for another is inversely as its saturation with it.

a. Healthy persons after breathing it accurately for a minute and a half, or two minutes, are usually exhilarated, and affected by strong pleasurable sensations, the circulation is quickened, the countenance becomes darker, and a sense of warmth is induced.

b. On unhealthy persons the effects are less pleasant, and in certain cases it has induced hysterical affections.

c. Nitrous oxide after being breathed for more than three minutes, produces violent excitement, which generally ends in momentary loss of sensation.

d. As far as experiments have yet gone, there is no reason to believe that any notable debility follows, even its most violent agency.

Sect. 2. Warm-blooded animals when permanently immersed in nitrous oxide, live longer in it than in any other gases, except such as contain free oxygen; but cold-blooded animals are quickly destroyed by it.

a. The smaller quadrupeds die in pure nitrous oxide in three or four minutes; they absorb a little of the gas, and their death seems to be occasioned by excessive stimulation.

b. When examined after death their internal parts are found to have undergone very peculiar changes.

The animal fibre is very irritable; the blood both in the arteries and veins is dark and purple, and the lungs are marked with large purple spots.

Sect. 3. When nitrous oxide is exposed to fluid venous blood, whether in the vessels or out of them, it is rapidly absorbed, apparently no portion of it is decomposed, and the colour of the blood becomes rather more inclined to purple.

II.

*Observations on the supposed Magnetic Property of Nickel, by RICHARD CHEVENIX, Esq.
F. R. S. and M. R. I. A. Communicated by the Author.*

THE properties of magnetism and polarity, first observed in iron, were, for a long time, the attributes of it alone. But the experiments of later philosophers had given reason to suppose, that nickel and cobalt participate of both. The celebrated Bergman, in his dissertation upon the former of these two metals, after lamenting the insufficiency of all his attempts to deprive it of the last portions of iron with which it was contaminated, concludes the paragraph with a doubt, whether magnetism may not be a property of nickel, as well as of iron. To this the authority of Arvidson may be added; and hence chymists have supposed, that magnetism is not a property of iron exclusively. In Italy, within these five years, as I have been informed, compasses for marine purposes have been made of nickel and of cobalt; thus being preferable to iron, as better resisting the action of the air. I know it was the opinion of the French chymists, that magnetic needles, made of
either

either of those metals perfectly pure, answered the purpose as well as those made of the usual substance. In the summer of 1798 I was witness to some experiments, performed at the *Ecole des mines*, to shew that several pieces of both, which had been reduced to the metallic state, in the laboratory of that establishment, and which were magnetic, were totally exempt from iron. The experiments were conducted by Mons. Vauquelin, and were as follows: some magnetic bits of each of those metals were dissolved in an acid, and iron was, in vain, looked for by the delicate tests of an infusion of gall nuts, and a solution of prussiate of pot-ash. Hence it was reasonable to conclude that no iron was present, and that nickel and cobalt really are magnetic of themselves, and independently of alloy.

If I have obtained nickel in its metallic state, and in no way attractable, it was not in performing experiments with this direct view, for so fully was I convinced of the accuracy of those I had witnessed in Paris in 1798, that I entertained no doubt upon the subject. I had an acid solution of impure oxide of nickel, which I precipitated by ammonia, for the purpose of procuring some pure nickel which I happened to want. I filtered immediately, and suffered the ammoniacal solution to stand some days in a well stopped bottle. The liquor remained of the usual colour, but a blueish precipitate, which likewise I separated by the filtre, had been formed at the bottom. The alkaline liquor, which had passed clear, was evaporated; and the oxide of nickel, which fell as the ammonia was volatilized, was collected, washed, dried, and treated with a proper flux, in a Hessian crucible, and in a forge capable of giving a strong heat. By this process I obtained a result which was nickel, so unattractable, that presented as near as a steady hand could hold it, to a very delicate magnet, supported on a cork floating on water or on mercury, no visible influence took place upon the magnet. A bit of the nickel, dissolved in an acid, exhibited, as may be well supposed, no traces of iron.

Its specific gravity, taken by the Count de Bournon, at a balance sensible to the $\frac{1}{250}$ of a grain, and at the temperature of 60, was 7,3806, a specific gravity much inferior to that which Bergman had found, 9,0000: and even to that given by Mons. de Morveau, 7,8070; hence it would appear that the presence of iron tends to increase its density. Its fusibility I could not ascertain, having no pyrometer. Its malleability is certainly greater than that of zinc. Its colour, like some of the alloys of silver and copper, of a very pale red. Its hardness considerable, and its fracture grainy and uneven. Several of my chymical friends in this country have witnessed the inefficacy of this nickel in attracting the magnetic needle; and Professor Pictet of Geneva was so good as to take some of it to the chymists in France, to convince them of the error which they are under, with regard to this property in nickel.

It does not appear from the results of the experiments to which, as I mentioned, I was a witness in 1798, that there was any inaccuracy in the mode of operating; but merely, that magnetism is a re-agent more powerful to detect minute portions of iron, when in its metallic state, than is either a solution of gall nuts, or of prussiate of pot-ash, to discover

its oxide when held in solution by acid menstua. Indeed the magnet will, in advantageous circumstances, be influenced by the smallest particle of metallic iron adhering to the surface of any body, without regard to the actual quantity of the substance to which it is attached; while the efficacy of chymical tests is, in some measure, bounded by the proportion of heterogeneous elements. In one case the action is independent and absolute; in the other, relative and limited. Thus a lump of copper, of whatever size, being struck with an iron hammer upon an anvil, sometimes acquires the power of influencing the magnet; but I believe it would be rather difficult to discover, by reagents, the iron that confers on it that power; and which perhaps does not amount to a millionth part of the whole mass.

As far as I have observed, the following cautions alone are necessary in thus treating the oxide of nickel by ammonia.

1. All oxides of iron, except the red, are soluble, more or less, in ammonia; therefore the iron must, by the means of nitric acid, be brought to its full state of oxidation, before precipitation and redissolution.

2. The ammonia must be perfectly caustic; for carbonate of ammonia dissolves red oxide of iron. And,

3. It may be expedient to allow some precipitate of nickel to be formed at the bottom, before the ammoniacal liquor is drawn off for evaporation; as, by that time, we may be sure, that if the two first conditions have been observed, all the oxide of iron has previously fallen to the bottom.

Although I have not directly performed a similar operation upon cobalt, or obtained that metal unattractable, I have little doubt but that such a quality is intirely owing to iron; and that the property of magnetism must be again restricted to its former limits.

III.

On the Mechanism of the Eye. By THOMAS YOUNG, M.D. F.R.S.

(Continued from page 265.)

THESE considerations will explain the inconsistency of different observations on the refractive power of the crystalline; and, in particular, how the refraction which I formerly calculated, from measuring the focal length of the lens,* is so much greater than that which is determined by other means. But, for direct experiments, Dr. Wollaston's method is exceedingly accurate:

* Phil. Transf. for 1793, p. 174.

When I look at a minute lucid point, such as the image of a candle in a small concave speculum, it appears as a radiated star, as a cross, or as an unequal line, and never as a perfect point, unless I apply a concave lens inclined at a proper angle, to correct the unequal refraction of my eye. If I bring the point very near, it spreads into a surface nearly circular, and almost equally illuminated, except some faint lines, nearly in a radiating direction. For this purpose, the best image is a candle, or a small speculum, viewed through a minute lens at some little distance, or seen by reflection in a larger lens. If any pressure has been applied to the eye, such as that of the finger keeping it shut, the sight is often confused for a short time after the removal of the finger, and the image is in this case spotty or curdled. The radiating lines are probably occasioned by some slight inequalities in the surface of the lens, which is very superficially furrowed in the direction of its fibres: the curdled appearance will be explained hereafter. When the point is further removed, the image becomes evidently oval, the vertical diameter being longest, and the lines a little more distinct than before, the light being strongest in the neighbourhood of the centre; but immediately at the centre there is a darker spot, owing to such a slight depression at the vertex as is often observable in examining the lens after death. The situation of the rays is constant, though not regular; the most conspicuous are seven or eight in number; sometimes about twenty fainter ones may be counted. Removing the point a little further, the image becomes a short vertical line; the rays that diverged horizontally being perfectly collected, while the vertical rays are still separate. In the next stage, which is the most perfect focus, the line spreads in the middle, and approaches nearly to a square, with projecting angles, but is marked with some darker lines towards the diagonals. The square then flattens into a rhombus, and the rhombus into a horizontal line unequally bright. At every greater distance, the line lengthens, and acquires also breadth, by radiations shooting out from it, but does not become a uniform surface, the central part remaining always considerably brightest, in consequence of the same flattening of the vertex which before made it fainter. Some of these figures bear a considerable analogy to the images derived from the refraction of oblique rays, (Schol. 4. Prop. IV.) and still more strongly resemble a combination of two of them in opposite directions; so as to leave no doubt, but that both surfaces of the lens are oblique to the visual axis, and co-operate in distorting the focal point. This may also be verified, by observing the image delineated by a common glass lens, when inclined to the incident rays. (See Plate XV. Fig. 28—40.)

The visual axis being fixed in any direction, I can at the same time see a luminous object placed laterally at a considerable distance from it; but in various directions the angle is very different. Upwards it extends to 50 degrees, inwards to 60, downwards to 70, and outwards to 90 degrees. These internal limits of the field of view nearly correspond with the external limits formed by the different parts of the face, when the eye is directed forwards and somewhat downwards, which is its most natural position; although the internal limits are a little more extensive than the external; and both are well calculated for enabling us to perceive the most readily, such objects as are the most likely to concern us.

Dr. Wollaston's eye has a larger field of view, both vertically and horizontally, but nearly in the same proportions, except that it extends further upwards. It is well known, that the retina advances further forwards towards the internal angle of the eye, than towards the external angle; but upwards and downwards its extent is nearly equal, and is indeed every way greater than the limits of the field of view, even if allowance is made for the refraction of the cornea only. The sensible portion seems to coincide more nearly with the painted choroid of quadrupeds: but the whole extent of perfect vision is little more than 10 degrees; or, more strictly speaking, the imperfection begins within a degree or two of the visual axis, and at the distance of 5 or 6 degrees becomes nearly stationary, until, at a still greater distance, vision is wholly extinguished. The imperfection is partly owing to the unavoidable aberration of oblique rays, but principally to the insensibility of the retina: for, if the image of the sun itself be received on a part of the retina remote from the axis, the impression will not be sufficiently strong to form a permanent spectrum, although an object of very moderate brightness will produce this effect when directly viewed. It would probably have been inconsistent with the economy of nature, to bestow a larger share of sensibility on the retina. The optic nerve is at present very large; and the delicacy of the organ renders it, even at present, very susceptible of injury from slight irritation, and very liable to inflammatory affections; and, in order to make the sight so perfect as it is, it was necessary to confine that perfection within narrow limits. The motion of the eye has a range of about 55 degrees in every direction; so that the field of perfect vision, in succession, is by this motion extended to 110 degrees.

But the whole of the retina is of such a form as to receive the most perfect image, on every part of its surface, that the state of each refracted pencil will admit; and the varying density of the crystalline renders that state more capable of delineating such a picture, than any other imaginable contrivance could have done. To illustrate this, I have constructed a diagram, representing the successive images of a distant object filling the whole extent of view, as they would be formed by the successive refractions of the different surfaces. Taking the scale of my own eye, I am obliged to substitute, for a series of objects at any indefinitely great distance, a circle of 10 inches radius; and it is most convenient to consider only those rays which pass through the anterior vertex of the lens; since the actual centre of each pencil must be in the ray which passes through the centre of the pupil, and the short distance of the vertex of the lens from this point, will always tend to correct the unequal refraction of oblique rays. The first curve (Plate XV. Fig. 16.) is the image formed by the furthest intersection of rays refracted at the cornea; the second, the image formed by the nearest intersection; the distance between these, shows the degree of confusion in the image; and the third curve, its brightest part. Such must be the form of the image which the cornea tends to delineate in an eye deprived of the crystalline lens; nor can any external remedy properly correct the imperfection of lateral vision. The next three curves show the images formed after the refraction at the anterior surface of the lens, distinguished in the same manner; and the three following, the result of all the successive

refractions. The tenth curve is a repetition of the ninth, with a slight correction near the axis, at F, where, from the breadth of the pupil, some perpendicular rays must fall. By comparing this with the eleventh, which is the form of the retina, it will appear that nothing more is wanting for their perfect coincidence, than a moderate diminution of density in the lateral parts of the lens. If the law, by which this density varies, were more accurately ascertained, its effect on the image might be estimated by means of the eighth proposition; and probably the image, thus corrected, would approach very nearly to the form of the twelfth curve.

To find the place of the entrance of the optic nerve, I fix two candles at ten inches distance, retire sixteen feet, and direct my eye to a point four feet to the right or left of the middle of the space between them: they are then lost in a confused spot of light; but any inclination of the eye brings one or the other of them into the field of view. In Bernoulli's eye, a greater deviation was required for the direction of the axis;* and the obscured part appeared to be of greater extent. From the experiment here related, the distance of the centre of the optic nerve from the visual axis is found (by Prop. V.) to be 16 hundredths of an inch; and the diameter of the most insensible part of the retina, one-thirtieth of an inch. In order to ascertain the distance of the optic nerve from the point opposite to the pupil, I took the sclerotica of the human eye, divided it into segments, from the centre of the cornea towards the optic nerve, and extended it on a plane. I then measured the longest and shortest distances from the cornea to the perforation made by the nerve, and their difference was exactly one-fifth of an inch. To this we must add a fiftieth, on account of the eccentricity of the pupil in the uvea, which in the eye that I measured was not great, and the distance of the centre of the nerve from the point opposite the pupil will be 11 hundredths. Hence it appears, that the visual axis is five hundredths, or one-twentieth of an inch, further from the optic nerve than the point opposite the pupil. It is possible that this distance may be different in different eyes; in mine, the obliquity of the lens, and the eccentricity of the pupil with respect to it, will tend to throw a direct ray upon it, without much inclination of the whole eye; and it is not improbable, that the eye is also turned slightly outwards, if looking at any object before it; although the inclination is too small to be subjected to measurement.

It must also be observed, that it is very difficult to ascertain the proportions of the eye so exactly, as to determine, with certainty, the size of an image on the retina; the situation, curvature, and constitution of the lens, make so material a difference in the result, that there may possibly be an error of almost one-tenth of the whole. In order, therefore, to obtain some confirmation from experiment, I placed two candles at a small distance from each other, turned the eye inwards, and applied the ring of a key so as to produce a spectrum, of which the edge coincided with the inner candle; then, fixing my eye on the outward one, I found that the spectrum advanced over two-sevenths of the distance between them.

* Comm. Petrop. I. p. 314.

Hence,

Hence, the same portion of the retina that subtended an angle of seven parts at the centre of motion of the eye, subtended an angle of five at the supposed intersection of the principal rays; (Plate XIV. Fig. 11.) and the distance of this intersection from the retina was 637 thousandths. This nearly corresponds with the former calculation; nor can the distance of the centre of the optic nerve from the point of most perfect vision be, on any supposition, much less than that which is here assigned. And, in the eyes of quadrupeds, the most strongly painted part of the choroid is further from the nerve than the real axis of the eye.

I have endeavoured to express in four figures, the form of every part of my eye, as nearly as I have been able to ascertain it; the first (Pl. XV. Fig. 17.) is a vertical section; the second (Fig. 18.) a horizontal section; the third and fourth are front views, in different states of the pupil. (Fig. 19 and 20.)

Considering how little inconvenience is experienced from so material an inequality in the refraction of the lens as I have described, we have no reason to expect a very accurate provision for correcting the aberration of the lateral rays. But, as far as can be ascertained by the optometer, the aberration arising from figure is completely corrected; since four or more images of the same line appear to meet exactly in the same point, which they would not do if the lateral rays were materially more refracted than the rays near the axis. The figure of the surfaces is sometimes, and perhaps always, more or less hyperbolical* or elliptical: in the interior laminae indeed, the solid angle of the margin is somewhat rounded off; but the weaker refractive power of the external parts, must greatly tend to correct the aberration arising from the too great curvature towards the margin of the disc. Had the refractive power been uniform, it might have collected the lateral rays of a direct pencil nearly as well; but it would have been less adapted to oblique pencils of rays; and the eye must also have been encumbered with a mass of much greater density than is now required, even for the central parts: and, if the whole lens had been smaller, it would also have admitted too little light. It is possible too, that Mr. Ramsden's observation, † on the advantage of having no reflecting surfaces, may be well-founded: but it has not been demonstrated, that less light is lost in passing through a medium of variable density, than in a sudden transition from one part of that medium to another; nor are we yet sufficiently acquainted with the cause of this reflection, to be enabled to reason satisfactorily on the subject. But, neither this gradation, nor any other provision, has the effect of rendering the eye perfectly achromatic. Dr. Jurin had remarked this, long ago, ‡ from observing the colour bordering the image of an object seen indistinctly. Dr. Wollaston pointed out to me on the optometer, the red and blue appearance of the opposite internal angles of the crossing lines; and mentioned, at the same time, a very elegant experiment for proving the dispersive power of the eye. He looks through a prism at a small lucid point, which of course

* Petit Mém. de l'Acad. 1725, p. 20.

† Phil. Trans. for 1795, p. 2.

‡ Smith, c. 96.

becomes

becomes a linear spectrum. But the eye cannot so adapt itself as to make the whole spectrum appear a line; for, if the focus be adapted to collect the red rays to a point, the blue will be too much refracted, and expand into a surface; and the reverse will happen if the eye be adapted to the blue rays; so that, in either case, the line will be seen as a triangular space. The observation is confirmed, by placing a small concave speculum in different parts of a prismatic spectrum, and ascertaining the utmost distances at which the eye can collect the rays of different colours to a focus. By these means I find, that the red rays, from a point at 12 inches distance, are as much refracted as white or yellow light at 11. The difference is equal to the refraction of a lens 132 inches in focus. But the aberration of the red rays in a lens of crown glass, of equal mean refractive power with the eye, would be equivalent to the effect of a lens 44 inches in focus. If, therefore, we can depend upon this calculation, the dispersive power of the eye collectively, is one-third of the dispersive power of crown glass, at an equal angle of deviation. I cannot observe much aberration in the violet rays. This may be, in part, owing to their faintness; but yet I think their aberration must be less than that of the red rays. I believe it was Mr. Ramsden's opinion, that since the separation of coloured rays is only observed where there is a sudden change of density, such a body as the lens, of a density gradually varying, would have no effect whatever in separating the rays of different colours. If this hypothesis should appear to be well-founded, we must attribute the whole dispersion to the aqueous humour; and its dispersive power will be half that of crown glass, at the same deviation. But we have an instance, in the atmosphere, of a very gradual change of density; and yet Mr. Gilpin informs me, that the stars, when near the horizon, appear very evidently coloured. At a more favourable season of the year, it would not be difficult to ascertain, by means of the optometer, the dispersive power of the eye, and of its different parts, with greater accuracy than by the experiment here related. Had the dispersive power of the whole eye been equal to that of flint glass, the distances of perfect vision would have varied from 12 inches to 7 for different rays, in the same state of the mean refractive powers.

VII. The faculty of accommodating the eye to various distances, appears to exist in very different degrees in different individuals. The shortest distance of perfect vision in my eye, is 26 tenths of an inch for horizontal, and 29 for vertical rays. This power is equivalent to the addition of a lens of 4 inches focus. Dr. Wollaston can see at 7 inches, and with converging rays; the difference answering to 6 inches focal length. Mr. Abernethy has perfect vision from 3 inches to 30, or a power equal to that of a lens $3\frac{1}{2}$ inches in focus. A young lady of my acquaintance can see at 2 inches and at 4; the difference being equivalent to 4 inches focus. A middle aged lady at 3 and at 4; the power of accommodation being only equal to the effect of a lens of 12 inches focus. In general, I have reason to think, that the faculty diminishes in some degree, as persons advance in life; but some also of a middle age appear to possess it in a very small degree. I shall take the range of my own eye, as being probably about the medium, and inquire what changes will be necessary

necessary in order to produce it; whether we suppose the radius of the cornea to be diminished, or the distance of the lens from the retina to be increased, or these two causes to act conjointly, or the figure of the lens itself to undergo an alteration.

1. We have calculated, that when the eye is in a state of relaxation, the refraction of the cornea is such as to collect rays diverging from a point ten inches distant, to a focus at the distance of $13\frac{2}{3}$ tenths. In order that it may bring to the same focus, rays diverging from a point distant 29 tenths, we find (by Cor. 5, Prop. IV.) that its radius must be diminished from 31 to 25 hundredths, or very nearly in the ratio of five to four.

2. Supposing the change from perfect vision at ten inches to 29 tenths, to be effected by a removal of the retina to a greater distance from the lens, this will require, (by the same Corollary,) an elongation of 135 thousandths, or more than one-seventh of the diameter of the eye. In Mr. Abernethy's eye, an elongation of 17 hundredths, or more than one-sixth, is requisite.

3. If the radius of the cornea be diminished one-sixteenth, or to 29 hundredths, the eye must at the same time be elongated 97 thousandths, or about one-ninth of its diameter.

4. Supposing the crystalline lens to change its form; if it became a sphere, its diameter would be 28 hundredths, and, its anterior surface retaining its situation, the eye would have perfect vision at the distance of an inch and a half. (Cor. 5 and 8, Prop. IV.) This is more than double the actual change. But it is impossible to determine precisely how great an alteration of form is necessary, without ascertaining the nature of the curves into which its surfaces may be changed. If it were always a spheroid more or less oblate, the focal length of each surface would vary inversely as the square of the axis: but, if the surfaces became, from spherical, portions of hyperbolic conoids, or of oblong spheroids, or changed from more obtuse to more acute figures of this kind, the focal length would vary more rapidly. Disregarding the elongation of the axis, and supposing the curvature of each surface to be changed proportionally, the radius of the anterior must become about 21, and that of the posterior 15 hundredths.

VIII. I shall now proceed to inquire, which of these changes takes place in nature; and I shall begin with a relation of experiments made in order to ascertain the curvature of the cornea in all circumstances.

The method described in Mr. Home's Croonian Lecture for 1795,* appears to be far preferable to the apparatus of the preceding year:† for a difference in the distance of two images seen in the cornea, would be far greater, and more conspicuous, than a change of its prominency, and far less liable to be disturbed by accidental causes. It is nearly, and perhaps totally impossible to change the focus of the eye, without some motion of its axis. The eyes sympathize perfectly with each other; and the change of focus is almost inseparable from a change of the relative situation of the optic axes; so much, that if I direct both my eyes at an object beyond their furthest focus, I cannot avoid bringing that focus a little

* Phil. Trans. for 1796, p. 2.

† Phil. Trans. for 1795, p. 13.

nearer: while one axis moves, it is not easy to keep the other perfectly at rest; and it is not impossible, that a change in the proportions of some eyes, may render a slight alteration of the position of the axis absolutely necessary. These considerations may partly explain the trifling difference in the place of the cornea that was observed in 1794. It appears that the experiments of 1795 were made with considerable accuracy, and no doubt with excellent instruments; and their failing to ascertain the existence of any change, induced Mr. Home and Mr. Ramsden to abandon, in great measure, the opinion which suggested them, and to suppose, that a change of the cornea produces only one-third of the effect. Dr. Olbers of Bremen, who in the year 1780 published a most elaborate dissertation on the internal changes of the eye,* which he lately presented to the Royal Society, had been equally unsuccessful in his attempts to measure this change of the cornea, at the same time that his opinion was in favour of its existence.

Room was however still left for a repetition of the experiments; and I began with an apparatus nearly resembling that which Mr. Home has described. I had an excellent achromatic microscope, made by Mr. Ramsden for my friend Mr. John Ellis, of five inches focal length, magnifying about 20 times. To this I adapted a cancellated micrometer, in the focus of the eye not employed in looking through the microscope: it was a large card, divided by horizontal and vertical lines into fortieths of an inch. When the image in the microscope was compared with this scale, care was taken to place the head so that the relative motion of the images on the micrometer, caused by the unsteadiness of the optic axis, should always be in the direction of the horizontal lines, and that there could be no error, from this motion, in the dimensions of the image taken vertically. I placed two candles so as to exhibit images in a vertical position in the eye of Mr. Konig, who had the goodness to assist me; and, having brought them into the field of the microscope, where they occupied 35 of the small divisions, I desired him to fix his eye on objects at different distances in the same direction; but I could not perceive the least variation in the distance of the images.

Finding a considerable difficulty in a proper adjustment of the microscope, and being able to depend on my naked eye in measuring distances, without an error of one 500th of an inch, I determined to make a similar experiment without any magnifying power. I constructed a divided eye-glass of two portions of a lens, so small, that they passed between two images reflected from my own eye; and, looking in a glass, I brought the apparent places of the images to coincide, and then made the change for viewing nearer objects; but the images still coincided. Neither could I observe any change in the images reflected from the other eye, where they could be viewed with greater convenience, as they did not interfere with the eye-glass. But, not being at that time aware of the perfect sympathy of the eyes, I thought it most certain to confine my observation to the one with which I saw. I

* *De Oculi Mutationibus internis.* Gotting. 1780. 4^{to}.

must remark that, by a little habit, I have acquired a very ready command over the accommodation of my eye, so as to be able to view an object with attention, without adjusting my eye to its distance.

I also stretched two threads, a little inclined to each other, across a ring, and divided them by spots of ink into equal spaces. I then fixed a ring, applied my eye close behind it, and placed two candles in proper situations before me, and a third on one side, to illuminate the threads. Then, setting a small looking-glass, first at four inches distance, and next at two, I looked at the images reflected in it, and observed at what part of the threads they exactly reached across in each case; and with the same result as before.

I next fixed the cancellated micrometer at a proper distance, illuminated it strongly, and viewed it through a pin-hole, by which means it became distinct in every state of the eye; and, looking with the other eye into a small glass, I compared the image with the micrometer in the manner already described. I then changed the focal distance of the eye, so that the lucid points appeared to spread into surfaces, from being too remote for perfect vision; and I noted on the scale, the distance of their centres; but that distance was invariable.

Lastly, I drew a diagonal scale, with a diamond, on a looking-glass, (Plate XIV. Fig. 12.) and brought the images into contact with the lines of the scale. Then, since the image of the eye occupies on the surface of a glass half its real dimensions, at whatever distance it is viewed, its true size is always double the measure thus obtained. I illuminated the glass strongly, and made a perforation in a narrow slip of black card, which I held between the images; and was thus enabled to compare them with the scale, although their apparent distance was double that of the scale. I viewed them in all states of the eye; but I could perceive no variation in the interval between them.

The sufficiency of these methods may be thus demonstrated. Make a pressure along the edge of the upper eyelid with any small cylinder, for instance a pencil, and the optometer will show that the focus of horizontal rays is a little elongated, while that of vertical rays is shortened; an effect which can only be owing to a change of curvature in the cornea. Not only the apparatus here described, but even the eye unassisted, will be capable of discovering a considerable change in the images reflected from the cornea, although the change be much smaller than that which is requisite for the accommodation of the eye to different distances. On the whole, I cannot hesitate to conclude, that if the radius of the cornea were diminished but one-twentieth, the change would be very readily perceptible by some of the experiments related; and the whole alteration of the eye requires one-fifth.

But a much more accurate and decisive experiment remains. I take out of a small botanical microscope, a double convex lens, of eight-tenths radius and focal distance, fixed in a socket one-fifth of an inch in depth; securing its edges with wax, I drop into it a little water, nearly cold, till it is three-fourths full, and then apply it to my eye, so that the cornea enters half way into the socket, and is every where in contact with the water. (Plate XIV. Fig. 13.) My eye immediately becomes presbyopic, and the refractive power

of the lens, which is reduced by the water to a focal length of about 16 tenths, (Cor. 5, Prop. IV.) is not sufficient to supply the place of the cornea, rendered inefficacious by the intervention of the water; but the addition of another lens, of five inches and a half focus, restores my eye to its natural state, and somewhat more. I then apply the optometer, and I find the same inequality in the horizontal and vertical refractions as without the water; and I have, in both directions, a power of accommodation equivalent to a focal length of four inches, as before. As first sight indeed, the accommodation appears to be somewhat less, and only able to bring the eye from the state fitted for parallel rays to a focus at five inches distance; and this made me once imagine, that the cornea might have some slight effect in the natural state; but, considering that the artificial cornea was about a tenth of an inch before the place of the natural cornea, I calculated the effect of this difference, and found it exactly sufficient to account for the diminution of the range of vision. I cannot ascertain the distance of the glass lens from the cornea to the hundredth of an inch; but the error cannot be much greater, and it may be on either side.

After this, it is almost necessary to apologize for having stated the former experiments; but, in so delicate a subject, we cannot have too great a variety of concurring evidence.

IX. Having satisfied myself that the cornea is not concerned in the accommodation of the eye, my next object was to inquire if any alteration in the length of its axis could be discovered; for this appeared to be the only possible alternative: and, considering that such a change must amount to one-seventh of the diameter of the eye, I flattered myself with the expectation of submitting it to measurement. Now, if the axis of the eye were elongated one-seventh, its transverse diameter must be diminished one-fourteenth, and the semi-diameter would be shortened a thirtieth of an inch.

I therefore placed two candles so that when the eye was turned inwards, and directed towards its own image in a glass, the light reflected from one of the candles by the sclerotica appeared upon its external margin, so as to define it distinctly by a bright line; and the image of the other candle was seen in the centre of the cornea. I then applied the double eye-glass, and the scale of the looking-glass, in the manner already described; but neither of them indicated any diminution of the distance, when the focal length of the eye was changed.

Another test, and a much more delicate one, was the application of the ring of a key at the external angle, when the eye was turned as much inwards as possible, and confined at the same time by a strong oval iron ring, pressed against it at the internal angle. The key was forced in as far as the sensibility of the integuments would admit, and was wedged, by a moderate pressure, between the eye and the bone. In this situation, the phantom caused by the pressure extended within the field of perfect vision, and was very accurately defined; nor did it, as I formerly imagined, by any means prevent a distinct perception of the objects actually seen in that direction; and a straight line coming within the field of this oval phantom, appeared somewhat inflected towards its centre; (Plate XIV. Fig. 14.)

a dis-

a distortion easily understood by considering the effect of the pressure on the form of the retina. Supposing now, the distance between the key and the iron ring to have been, as it really was, invariable, the elongation of the eye must have been either totally or very nearly prevented; and, instead of an increase of the length of the eye's axis, the oval spot caused by the pressure would have spread over a space at least ten times as large as the most sensible part of the retina. But no such circumstance took place: the power of accommodation was as extensive as ever; and there was no perceptible change, either in the size or in the figure of the oval spot.

Again, since the rays which pass through the centre of the pupil, or rather the anterior vertex of the lens, may, as already observed, be considered as delineating the image; and, since the divergence of these rays with respect to each other, is but little affected by the refraction of the lens, they may still be said to diverge from the centre of the pupil; and the image of a given object on the retina must be very considerably enlarged, by the removal of the retina to a greater distance from the pupil and lens. (Cor. Prop. V*.) To ascertain the real magnitude of the image with accuracy, is not so easy as it at first sight appears; but, besides the experiment last related, which might be employed as an argument to this purpose, there are two other methods of estimating it. The first is too hazardous to be of much use; but, with proper precautions, it may be attempted. I fix my eye on a brass circle placed in the rays of the sun, and, after some time, remove it to the cancellated micrometer; then, changing the focus of my eye, while the micrometer remains at a given distance, I endeavour to discover whether there is any difference in the apparent magnitude of the spectrum on the scale; but I can discern none. I have not insisted on the attempt; especially as I have not been able to make the spectrum distinct enough without inconvenience; and no light is sufficiently strong to cause a permanent impression on any part of the retina remote from the visual axis. I therefore had recourse to another experiment. I placed two candles so as exactly to answer to the extent of the termination of the optic nerve, and, marking accurately the point to which my eye was directed, I made the utmost change in its focal length; expecting that, if there were any elongation of the axis, the external candle would appear to recede outwards upon the visible space. (Plate XIV. Fig. 15.) But this did not happen; the apparent place of the obscure part was precisely the same as before. I will not undertake to say, that I could have observed a very minute difference either way: but I am persuaded, that I should have discovered an alteration of less than a tenth part of the whole.

It may be inquired if no change in the magnitude of the image is to be expected on any other supposition; and it will appear to be possible, that the changes of curvature may be

* This Corollary should stand thus. "If a confused image be received on any given plane, it will be necessary, in order to determine its magnitude, to advert to the aperture admitting the rays. If the aperture be supposed to be infinitely small, it may be considered as a radiant point, in order to find the direction of the emergent rays."

so adapted, that the magnitude of the confused image may remain perfectly constant. Indeed, to calculate from the dimensions which we have hitherto used, it would be expected, that the image should be diminished about one-fortieth, by the utmost increase of the convexity of the lens. But the whole depends on the situation of the refracting surfaces, and the respective increase of their curvature, which, on account of the variable density of the lens, can scarcely be estimated with sufficient accuracy. Had the pupil been placed before the cornea, the magnitude of the image must, on any supposition, have been very variable: at present, this inconvenience is avoided by the situation of the pupil; so that we have here an additional instance of the perfection of this admirable organ.

From the experiments related, it appears to be highly improbable that any material change in the length of the axis actually takes place; and it is almost impossible to conceive by what power such a change could be effected. The straight muscles, with the adipose substance lying under them, would certainly, when acting independently of the socket, tend to flatten the eye: for, since their contraction would necessarily lessen the circumference or superficies of the mass that they contain, and round off all its prominences, their attachment about the nerve and the anterior part of the eye must therefore be brought nearer together. (Plate XV. Fig. 21, 22.) Dr. Olbers compares the muscles and the eye to a cone, of which the sides are protruded, and would by contraction be brought into a straight line. But this would require a force to preserve the cornea as a fixed point, at a given distance from the origin of the muscles; a force which certainly does not exist. In the natural situation of the visual axis, the orbit being conical, the eye might be somewhat lengthened, although irregularly, by being forced further into it; but, when turned towards either side, the same action would rather shorten its axis; nor is there any thing about the human eye that could supply its place. In quadrupeds, the oblique muscles are wider than in man; and in many situations might assist in the effect. Indeed a portion of the orbicular muscle of the globe is attached so near to the nerve, that it might also co-operate in the action: and I have no reason to doubt the accuracy of Dr. Olbers, who states, that he effected a considerable elongation, by tying threads to the muscles, in the eyes of hogs and of calves; yet he does not say in what position the axis was fixed; and the flaccidity of the eye after death might render such a change very easy as would be impossible in a living eye. Dr. Olbers also mentions an observation of Professor Wrisberg, on the eye of a man whom he believed to be destitute of the power of accommodation in his life-time, and whom he found, after death, to have wanted one or more of the muscles: but this want of accommodation was not at all accurately ascertained. I measured, in the human eye, the distance of the attachment of the inferior oblique muscle from the insertion of the nerve: it was one-fifth of an inch; and from the centre of vision not a tenth of an inch; so that, although the oblique muscles do in some positions nearly form a part of a great circle round the eye, their action would be more fitted to flatten than to elongate it. We have therefore reason to agree with Winflow, in attributing to them the

the office of helping to support the eye on that side where the bones are most deficient: they seem also well calculated to prevent its being drawn too much backwards by the action of the straight muscles. And, even if there were no difficulty in supposing the muscles to elongate the eye in every position, yet at least some small difference would be expected in the extent of the change, when the eye is in different situations, at an interval of more than a right angle from each other; but the optometer shews that there is none.

Dr. Hofack alleges that he was able, by making a pressure on the eye, to accommodate it to a nearer object: * it does not appear that he made use of very accurate means of ascertaining the fact; but, if such an effect took place, the cause must have been an inflection of the cornea.

It is unnecessary to dwell on the opinion which supposes a joint operation, of changes in the curvature of the cornea and in the length of the axis. This opinion had derived very great respectability, from the most ingenious and elegant manner in which Dr. Olbers had treated it, and from being the last result of the investigation of Mr. Home and Mr. Ramsden. But either of the series of experiments which have been related, appears to be sufficient to confute it.

X. It now remains to inquire into the pretensions of the crystalline lens to the power of altering the focal length of the eye. The grand objection to the efficacy of a change of figure in the lens, was derived from the experiments in which those who have been deprived of it have appeared to possess the faculty of accommodation.

My friend Mr. Ware, convinced as he was of the neatness and accuracy of the experiments related in the Croonian Lecture for 1795, yet could not still help imagining, from the obvious advantage all his patients found, after the extraction of the lens, in using two kinds of spectacles, that there must, in such cases, be a deficiency in that faculty. This circumstance, combined with a consideration of the directions very judiciously given by Dr. Porterfield, for ascertaining the point in question, first made me wish to repeat the experiments upon various individuals, and with the instrument which I have above described as an improvement of Dr. Porterfield's optometer: and I must here acknowledge my great obligation to Mr. Ware, for the readiness and liberality with which he introduced me to such of his numerous patients as he thought most likely to furnish a satisfactory determination. It is unnecessary to enumerate every particular experiment; but the universal result is, contrary to the expectation with which I entered on the inquiry, that in an eye deprived of the crystalline lens, the actual focal distance is totally unchangeable. This will appear from a selection of the most decisive observations.

1. Mr. R. can read at four inches and at six only, with the same glass. He saw the double lines meeting at three inches, and always at the same point; but the cornea was somewhat irregularly prominent, and his vision not very distinct; nor had I, at the time I saw him, a convenient apparatus.

* Phil. Trans. for 1794, p. 212.

I afterwards provided a small optometer, with a lens of less than two inches focus, adding a series of letters, not in alphabetical order, and projected into such a form as to be most legible at a small inclination. The excess of the magnifying power had the advantage of making the lines more divergent, and their crossing more conspicuous; and the letters served for more readily naming the distance of the intersection, and, at the same time, for judging of the extent of the power of distinguishing objects too near or too remote for perfect vision. (Plate XV. Fig. 23.)

2. Mr. J. had not an eye very proper for the experiment; but he appeared to distinguish the letters at $2\frac{1}{2}$ inches, and at less than an inch. This at first persuaded me, that he must have a power of changing the focal distance: but I afterwards recollected that he had withdrawn his eye considerably, to look at the nearer letters, and had also partly closed his eyelids, no doubt contracting at the same time the aperture of the pupil; an action which, even in a perfect eye, always accompanies the change of focus. The slider was not applied.

3. Miss H. a young lady of about twenty, had a very narrow pupil, and I had not an opportunity of trying the small optometer: but, when she once saw an object double through the slits, no exertion could make it appear single at the same distance. She used for distant objects a glass of $4\frac{1}{2}$ inches focus; with this she could read as far off as 12 inches, and as near as five: for nearer objects she added another of equal focus, and could then read at 7 inches, and at $2\frac{1}{2}$.

4. Hanson, a carpenter, aged 63, had a cataract extracted a few years since from one eye: the pupil was clear and large, and he saw well to work with a lens of $2\frac{3}{8}$ inches focus; and could read at 8 and at 15 inches, but most conveniently at 11. With the same glass, the lines of the optometer appeared always to meet at 11 inches; but he could not perceive that they crossed, the line being too strong, and the intersection too distant. The experiment was afterwards repeated with the small optometer: he read the letters from 2 to 3 inches; but the intersection was always at $2\frac{1}{2}$ inches. He now fully understood the circumstances that were to be noticed, and saw the crossing with perfect distinctness: at one time, he said it was a tenth of an inch nearer; but I observed that he had removed his eye two or three tenths from the glass, a circumstance which accounted for this small difference.

5. Notwithstanding Hanson's age, I consider him as a very fair subject for the experiment. But a still more unexceptionable eye was that of Mrs. Maberly. She is about 30, and had the crystalline of both eyes extracted a few years since, but sees best with her right. She walks without glasses; and, with the assistance of a lens of about four inches focus, can read and work with ease. She could distinguish the letters of the small optometer from an inch to $2\frac{1}{2}$ inches; but the intersection was invariably at the same point, about 19 tenths of an inch distant. A portion of the capsule is stretched across the pupil, and causes her to see remote objects double, when without her glasses; nor can she, by
any

any exertion, bring the two images nearer together, although the exertion makes them more distinct, no doubt by contracting the pupil. The experiment with the optometer was conducted, in the presence of Mr. Ware, with patience and perseverance; nor was any opinion given to make her report partial.

Considering the difficulty of finding an eye perfectly suitable for the experiments, these proofs may be deemed tolerably satisfactory. But, since one positive argument will counterbalance many negative ones, provided it be equally grounded on fact, it becomes necessary to inquire into the competency of the evidence employed to ascertain the power of accommodation attributed, in the Croonian Lecture for 1794, to the eye of Benjamin Clerk. And it appears, that the distinction long since very properly made by Dr. Jurin, between distinct vision and perfect vision, will readily explain away the whole of that evidence.

It is obvious that vision may be made distinct to any given extent, by means of an aperture sufficiently small, provided at the same time, that a sufficient quantity of light be left, while the refractive powers of the eye remain unchanged. And it is remarkable, that in those experiments, when the comparison with the perfect eye was made, the aperture of the imperfect eye only was very considerably reduced. Benjamin Clerk, with an aperture of $\frac{3}{40}$ of an inch, could read with the same glass at $1\frac{1}{2}$ inch, and at 7 inches.* With an equal aperture, I can read at $1\frac{1}{2}$ inch and at 30 inches: and I can retain the state of perfect relaxation, and read with the same aperture at $2\frac{1}{4}$ inches; and this is as great a difference as was observed in Benjamin's Clerk's eye. It is also a fact of no small importance, that Sir Henry Englefield was much astonished, as well as the other observers, at the accuracy with which the man's eye was adjusted to the same distance, in the repeated trials that were made with it,† This circumstance alone makes it highly probable, that its perfect vision was confined within very narrow limits.

(To be continued.)

IV.

Description of a Support applicable to Balances of all Dimensions, and proper to render the Operations performed with these Instruments more expeditious and convenient without affecting their Precision. ‡ By R. PRONY, of the National Institute of France.

SOME experiments which I was engaged in during the course of last winter obliged me to contrive a support, to which the beam of any balance whatever might be applied without distinction, whether provided with its frame of support or not, and which, without de-

* Phil. Transf. for 1795, p. 9.

† Phil. Transf. for 1795, p. 8.

‡ Annales de Chimie, XXXVI. 50.

prising it in any respect of its precision, should afford a convenient means of raising and lowering it at pleasure. It is well known how troublesome it is to weigh by balances held in the hand, and this trouble is frequently by no means the smallest inconvenience attending that method.

Several artists have invented supports, convenient in their use, and ingenious in their contrivance; but each of these is adapted only to a single balance, and increases the price in such a manner as to put it out of the convenience of most philosophers and artists. I hope I shall therefore give them pleasure by publishing, in consequence of the invitation of several celebrated chemists, the description of a support, which, to the advantage of being universal, adds that of cheapness and easy execution, whether in wood or in metal.

It was my intention to have added to this description some views respecting the theory and construction of balances, from which I apprehend that artists acquainted with the elements of geometry and calculation might derive some advantage in the improvement of machines for weighing; but as this part of my work is more extended than I at first thought, I shall publish it separately.

A triangular foot of brass, $A a, a, a,$, (Plate XVI. Fig. 1 & 2) has its three extremities, a, a'', a''' , solidly screwed to a table or horizontal plane. In the part A of this foot is screwed a cylindrical stem AB , of any convenient length. It may be proper to have two of different lengths, in order to exchange them when very large balances are used. Those which I have used are of half a metre and a metre in length.

A vertical pulley is placed at the top of the rod AB , so that the same vertical plane passes through the axis of the beam and the horizontal axis of the pulley. The support CD of this pulley has at its lower part a cylindrical bore, into which the upper extremity of the rod AB enters with an easy friction; and a thumb-screw E serves by its pressure to keep the pulley in its fixed position.

Another pulley P is fixed below the rod AB , so that the tangent line to the pulleys P & p is parallel to the axis of the rod AB .

A string $K t p H G P F$, at the end of which is suspended, beyond the edge of the table K , a small weight, k passes through a hole t , made in the foot a'' , thence over the pulleys p and P , and is fastened at F to the upper part of a piece $m m' n q$, which is forked, and to which are suspended, as I shall proceed to explain, the balance, the weights, and the objects to be weighed. $F m$ is a button, which, being screwed to the top of the fork, receives the string through a hole, in which it remains by virtue of a knot made in its extremity.

The tail, or lower part of this fork, is prismatic in its part $m' n$; and this prismatic part enters into a notch ff made in the extremity N of the horizontal piece NO , in which it can freely slide upwards and downwards through a limited space, its ascent being stopped by the enlargement of the fork below, and above by the enlargement occasioned by the separation of the two branches of the fork.

The piece *N O* is perforated, and may be slid up and down the stem *A B*, and also moved horizontally to the right and left. When it has the proper height, it is fixed, by means of the pressing screw *V*, that is to say, 1st. when its height is such that the stop *m* rests on the edge of the notch *f f*, or when *N O*, being at its lowest limit, the dishes of the balance are in contact with the table or horizontal plane, so that it may be raised afterwards through the whole height *f n*; and, 2^{dly}, that the string *F F* shall be in the same vertical plane with *H G*.

The notch in *N* must be so placed that the axis of the prismatic part of the tail of the fork and the string *F F'* shall be always in the same vertical line parallel to the axis of the stem *A B*.

These arrangements being understood, let us imagine the two branches *n q* of the fork to be drilled through with holes of different sizes, to receive the horizontal pins *g g* of different diameters, and we shall possess an apparatus quite adequate to weighing in the air with balances which have their beams supported between the usual frame and rings.

Whatever, in fact, may be the size of the balance intended to be used, the extremity of its clip, or frame, is to be introduced between the fork *n q*, and a pin of such a size as to enter very freely into the hole at the top of the clip must be passed through; after which the piece *O N* is to be placed, so that the conditions before mentioned with regard to this piece shall be obtained; and in this position it is to be fixed with the thumb-screw *V*. This operation being made, the dishes of the balance are to be loaded, which, being in contact with the table or horizontal plane, cannot move. The small ball *k* being then taken in the hand, the string must be drawn so that the balance shall rise very slowly. If the scales be not in equilibrium, the beam must be suffered to descend, and the process of weighing be completed by altering the load as usual.

A counterpoise *Q*, suspended on the string *F G*, forms an equilibrium to the weight of the balance. By means of this precaution, the result of all the efforts which support the pulley *P* passes in all cases through the axis of the rod *A B*, which, consequently, will have no disposition to bend or incline.

But if it be proposed to use an hydrostatical balance, a small table *O' N*, Fig. 3, is to be adapted to the rod *A B*, which, by means of a cylindrical perforation in *O'*, can slide up and down the rod, and be fixed at any required height by the thumb-screw *V'*. Another piece, or little table, *K' K*, is placed upon *V' N*, so that the holes *T* correspond to the centre of the dishes, below which are placed the hooks intended to hold the body suspended in water, and *K' K* is fixed upon *V' N* by means of the screw *V'*.

This adjustment being prepared, the piece *N O* is to be placed on the table *K' K*, so that, 1st. the whole height of the balance shall be comprehended between this piece and the small table, and the dishes shall be contact with the same, their centers corresponding with the openings made in *T*; 2^d. that *K' K* shall be sufficiently raised to admit of the vessels *W W* of water being placed beneath, so as conveniently to immerse the body intended to be weighed in one of these vessels.

These bodies are usually suspended by a very thin metallic wire; but, by placing two vessels as I have done, and suspending to each of the basons a wire of metal of the same diameter, one to support the body, and the other to be simply immersed in the water, the magnitude of the diameter will have no influence on the accuracy of the experiment. In fact, if we suppose the machine to be so adjusted that at the commencement of the operation the dishes shall be in equilibrio (which is easily effected by varying the height of the water in the vessels), these two wires will continue to form an equilibrium, when the beam $F F''$ being raised still preserves its horizontal position. Whence it follows that if to one of the wires there be suspended an immersed body, and in the opposite bason there be placed out of the water a weight capable of maintaining the equilibrium, that equilibrium will continue, whatever may be the elevation or depression of the whole balance, provided it continues horizontal. For the lengths of the wires raised out of the water being equal, the difference between the specific gravities of the water and the metal will have the same influence on the two extremities of the beam. It is evident that the same advantage could not be had by employing only one wire; namely, that which supports the suspended body, and that the equilibrium corresponding with a certain elevation, and the horizontal position of the beam, will not agree with other elevations and the same horizontal position.

It must be observed that my process not only compensates for the excess of specific gravity of the wires beyond that of the water, but also for the adhesion of the fluid and that portion which hangs round the wires, and is raised with them.

The whole of the preceding account is applicable to balances suspended in the usual clip, or forked piece, terminating in rings for supporting the fulcrum. But in order to render my support absolutely universal, it must be adapted to a beam alone, the knife edges of which shall find a proper place to act upon: and in this I have succeeded, by contriving a forked piece, which hangs from the fork nq , and is capable of receiving the knife edges of any beam whatever. In the drawing this piece is properly placed in the machine, and its construction is as follows:

A piece $S s'$ (fig. 2.) is pierced at s' by a tapped hole, into which the screw dd enters to half its length. Another hole made in S , perpendicular to the former, receives the pin gg , (fig. 1.) which suspends the whole lower apparatus, and answers to the hole in the upper part of the fork of common balances.

Two other vertical pieces rr , Fig. 4, have at their upper part holes not tapped, in which the screw dd may freely move. These upper ends are to be placed at an arbitrary distance, where they are retained, and fixed by four screw-nuts u , each piece being thus confined between two screws. A cylindrical rod bb passes through the lower extremities of these pieces rr , and is fixed by pressing screws; so that the upper and lower extremities of those pieces are invariably at the same distance.

Each of these pieces rr has on its face, which is perpendicular to the direction of dd and bb , a long opening ff , and a circular perforation X , (fig. 1. and 5.) the lower part of which is armed with a small face of polished steel, upon which the edge of the fulcrum rests;

rests; in the upper part of the openings *ff*, a rule *e' e'* is introduced by tight friction, which, together with the pieces *dd* and *bb*, give to the frame such a degree of solidity, that there is not the least shake or play in any of its parts. That part of its opening which is not occupied by *e' e'* must be rather longer than the longest index of the beams intended to be used.

The use of the frame or fork of support, here described, is extremely simple. The beam is to be placed between the two branches *rr*, which are to be separated till the knife edges are opposite the circular openings *X*, when they are again to be brought together, so as to include that fulcrum, leaving a little play between the supports, in order that the vibration may be perfectly free. The parallelism of the pieces *rr*, and the perfect solidity of their connection, is then to be effected by means of the screw-nuts, the small cylindric rod *bb*, and the rule *ee*. When these dispositions are made, the whole apparatus is to be suspended by the pin *gg*, in the fork *s*, and the balance is fit for use. The equilibrium, or horizontal position of the beam, is known by the position of the index *yy* in the middle of the opening *ee*; or, to render this estimate more exact, I have suspended to the rule *ee* a plumb-line *ei*, by which the slightest deviations from the perpendicular may be seen.

Such is the description and use of my support, which in practice has perfectly answered my expectation. The instrument I possess is entirely of brass, made by Citizen Dumotier, an artist well known for the construction of philosophical instruments. If the fork, and some of the nicer parts, be made of metal, the rest may be advantageously formed of wood.

V.

On the Combinations of the Metals with Sulphur. By the CITIZEN VAUQUELIN.

THE combinations of the metals with sulphur may be divided into three orders, namely,
1. The metals and sulphur. These are combinations, which properly speaking, ought to be called metallic sulphurets, or sulphurated metals; nature and art afford many specimens of these.

2. Metallic oxides and sulphur. These ought to be called sulphurated metallic oxides, or oxidized metallic sulphurets. They are also very common in the native state.

3. Metallic oxides with sulphur and hydrogen. These triple combinations are named hidro-sulphurated metallic oxides.

If the predominating substance in these different combinations be taken as the basis of the nomenclature, it would be necessary to reverse the order of the common denominations; for in the simple sulphurets, the metal always predominates as to its mass; the same is also the case in the sulphurated oxides; and in the hidro-sulphurets the predominance is first in the metals, then in the sulphur, and lastly, in the hydrogen.

So that instead of metallic fulphurets, oxidéd metallic fulphurets, and metallic hidrophulphurets; we ought to say sulphurated metals, sulphurated metallic oxides, and sulphurohydrogenated metallic oxides.

But usage has prevailed in chemistry, whether from facility of pronunciation, or the greater smoothness of the terms. However this is of no great importance, and if the elements of bodies be clearly understood from the terms, it will be sufficient.

Concerning the action of the acids upon the metallic fulphurets.

SULPHURIC ACID.

Whenever the sum of the affinities of oxygen for the metallic substance, and of the oxide thus formed for the sulphuric acid, is greater than that of the metal for sulphur and of oxygen for the sulphureous acid, there will constantly be a decomposition of the metallic fulphuret, and the result will be a metallic sulphate and sulphureous acid. The whole of these conditions are not always necessary: but each concurs in the accomplishment of the new combinations which are formed, and accelerates the operation. There are nevertheless cases, in which for want of one of these conditions the bodies do not act at all upon each other.

The sulphuric acid for example, decomposes the natural sulphurated oxide of iron or martial pyrites very well; but the muriatic acid cannot decompose it, because the sulphur in this case is not attracted by any body, whereas in the former it is attracted by the sulphuric acid, which it converts into sulphureous acid; for though I have found by many facts, that the iron in martial pyrites is oxidéd nearly to the point in which it is easily soluble in acids, it cannot however be decomposed without forming a great quantity of sulphuric acid.

This opinion is confirmed by the conditions necessary for completing the operation. In fact, the complete decomposition of pyrites cannot be effected without heat, and a great quantity of concentrated sulphuric acid. For it is evident here, that acid is not only required for the saturation of the oxide of iron, but also for that of the sulphur. I could likewise urge in support of the same opinion, the greater affinity of the muriatic acid for the oxide of iron, which nevertheless it is incapable of separating from the sulphur under these circumstances. It may seem strange that the nitric acid, of which the affinity to iron is less strong, should nevertheless decompose the martial pyrites with so much facility; but we ought not to deceive ourselves by supposing, that it is by a stronger affinity that this acid takes the oxide of iron from sulphur; for it is on the contrary, by affording a new quantity of oxygen to the iron that the affinity of the metal for the sulphur becomes singularly diminished. In reality, sulphur does not unite with iron oxidéd to the maximum, and when by certain peculiar processes these two substances are combined, the effect always happens from a separation of part of the oxygen.

Though the muriatic acid cannot decompose the sulphurated oxide of iron, it decomposes on the contrary the fulphuret of iron, in which the metal is not oxygenated, with the greatest facility. But in this case three forces act at once—namely, that of the iron on

the oxygen of the water, that of the sulphur on the hydrogen of the same substance, and lastly, that of the metallic oxide formed, for the muriatic acid. In this circumstance sulphurated hydrogen gas is constantly disengaged.

The sulphuric acid which does not decompose the sulphurated oxide of iron, unless it be concentrated and boiling, decomposes the sulphuret of iron without heat, and the more readily when it is diluted with a certain quantity of water. The reason of this evidently requires no explanation.

Though a great quantity of water opposes the action of the acids on the metallic sulphurets in general, nevertheless it is less noxious with respect to that of iron than with regard to the others, because the sulphurated hydrogen does not decompose the solutions of iron in the acids.

Concerning the Sulphuret of Lead.

In the natural sulphuret of lead or galena, lead exists in the metallic state; for which reason, when this mineral substance is brought into contact with the muriatic acid, sulphurated hydrogen is formed even in the cold. It appears even when the sulphur is united to the oxide of lead; this metal loses its oxygen in order to unite with the sulphur, for we obtain a matter perfectly similar to natural galena. The manner in which the reduction of lead is here effected may be easily understood. A portion of sulphur combines with the oxygen and forms sulphuric acid, which the heat volatilizes, and another portion unites with the metallic lead. The reduction of lead and the formation of galena take place nearly in the same manner, when a solution of this metal in an acid is mixed with a solution of sulphurated hydrogen, or of any hydro-sulphuret whatever. There is nevertheless this difference, namely, that in this case the hydrogen takes the oxygen from the lead, and forms water at the same time that the sulphur and the metal, which meet in a state of extreme division, combine without heat in the midst of the fluid. There is also another difference, that the galena formed by this process contains rather more sulphur than the natural compound, as is proved by the small quantity of sulphur which separates from this substance at a very gentle heat, and then leaves it absolutely in the same state as common galena.

From these facts it follows that the muriatic acid, and probably all other acids act on the oxide of lead by their mass and their concentration; that when these two forces, or even one of them is destroyed, or simply weakened, the sulphurated hydrogen deprives them of the lead; but whatever may be the quantity and concentration of the acid united with the lead or other metals, the decomposition of the salt will take place if an hydro-sulphuret be employed in sufficient quantity.

It is not singular that the affinities of the acids for the metallic oxides should be weakened by water, nor that their action on these substances should be more energetic by virtue of their masses. Antimony, mercury, bismuth, and lead itself, afford striking examples of this.

It is known, in fact, that when a large quantity of water is added to their concentrated solutions, very considerable precipitates are formed, which have been mistakenly supposed to be pure metallic oxides, but which are really salts, containing a portion of acid strongly adherent, that cannot be completely separated but by means of an alkali.

So that we may consider the combinations formed by these metals, and probably many others also with the acids, as neutral salts dissolved by a super-abundance of acid, which is taken from them by the water, and consequently the salts fall down truly neutral and insoluble.

From the manner in which the muriatic acid acts on the metallic sulphurets, it follows, that the more the metals which compose them absorb of oxygen to unite with this acid, the less there remains of sulphur after the solution, and *vice versa*; but if these metals absorb little oxygen, if they contain much sulphur, and if also their affinity for oxygen be inconsiderable, the totality of the metal will never be separated from the sulphur by the muriatic acid, because in proportion as this combustible body becomes more abundant, it opposes by its mass the solution of the metal, and hence it is that certain metallic sulphurets which are not attacked by the muriatic acid, yield to that force, after a portion of their sulphur has been separated by distillation.

The mode of action of the nitric acid on the metallic sulphurets is very different, and though the sulphur is not in this case attracted by any substance, nevertheless it is completely separated from the metal, and falls down perfectly pure if the operation have been properly performed; but though the combination of the nitric acid with the metal is not assisted by hydrogen, as happens with regard to the muriatic acid, yet this first acid yields oxygen to the metal more easily than water; and it is by virtue of this affinity of oxygen for the metal and the oxide which is formed for the undecomposed acid, that the solution is effected. So that in all the instances when metallic sulphurets are decomposed by the nitric acid, there is constantly a formation of nitrous gas, and never of sulphurated hydrogen.

It appears in general, that sulphur has a stronger affinity with the metals than with their oxides, and that this affinity diminishes in proportion to the quantity of oxygen they absorb. There are even some which do not unite with sulphur but in the complete metallic state. Thus for example, by triturating the brown oxide of lead with sulphur, a portion of the sulphur takes fire, and the other portion combines with the lead thus reduced to the metallic state.

It appears also that zinc does not unite with sulphur when it is completely metallic, or at least that nature has not yet presented instances of this, nor has art succeeded in combining these two substances. That which is known in mineralogy by the name of Blende, or sulphurated zinc, has been mistakenly regarded by some mineralogists as a combination of zinc with sulphur. It is on the contrary a sulphurated oxide of zinc, which very frequently contains hydrogen; for the blendes are almost always soluble in acids, and produce sulphurated

rated hydrogen gas. A transparent blende which acts in this manner with the acids, must therefore be considered as the true hidro-sulphurated oxide of zinc, or sulphuro-hydrogenated oxide. There may exist oxides of zinc simply sulphurated, and there would then be two species of ores very different, one soluble in the sulphuric and muriatic acids without effervescence, and the other on the contrary affording a greater or less quantity of sulphurated hydrogen gas.

Metallic mercury does not unite very intimately with sulphur. Mineral ethiops appear rather to be a mixture than a true combination, but when this oxidized metal is combined with sulphur, as for example in cinnabar, it adheres so strongly that no substance can directly decompose it, and the separation can never be effected but by altering one or other of these principles. It is either necessary to burn the sulphur, or dis-oxide the mercury. Cinnabar owes its fine red colour to the great quantity of oxygen which the mercury contains.

VI.

Notice respecting the extensive Logarithmic and Trigonometrical Tables, calculated under the Direction of CITIZEN PRONY, Member of the National Institute, &c. and of the Report concerning the same. By Citizens La Grange, Laplace, and Delambre.

WHEN in the year 2 of the Republic, C. Prony was charged to calculate the new tables of sines, tangents, &c. and their logarithms as required for the decimal division of the quadrant, he was engaged *not only to compose tables which should leave nothing to be desired with regard to accuracy, but to make them the most extended and most striking monument of calculation which had ever been executed, or even imagined.*

Cit. Prony applied to the execution of this grand enterprize suggested by Citizens Carnot, Prieur de la Côte d'Or, Brunet de Montpellier, which required to be finished in a short time, the principle of the division of labour by means of which the perfection of workmanship, with the saving of expence and time, are obtained in the arts. This division, which renders the most unskilful hands profitable in manual works, permits the useful employ of men of very limited powers in operations, which on the whole are delicate, but abound with simple and minute details. It is much to be wished, that this expedient might continue to be used for the perfection and extent of great scientific works, of which the plan cannot be understood but by men of a superior order, but of which the execution prodigiously exceeds their physical power. Such for example, are the numerical development of formulæ, which give the perturbation arising from the mutual attraction of different bodies of the planetary system, and the reduction of the astronomical observations proper to determine the elements of the orbits of these bodies.

Citizen.

Citizen Prony divided his assistants into three sections.

The first was composed of five or six mathematicians of very great merit, who were employed on the analytical part of the work, and in general on the application of the method of differences to the formation of tables, the calculation of various fundamental numbers, &c. Citizen Legendre, who for some time belonged to this section, gave very elegant formulæ for determining the successive differences of the sines *a priori*.

The second section consisted of calculators to whom the analysis was familiar. They directly calculated the differences of the several orders necessary to form by successive additions, according to the method of interpolation, the values of the trigonometric lines departing from a given first value through the whole interval, wherein the differences of the most elevated order might, without sensible error, be considered as constant.

The filling of these intervals was reduced to simple addition performed by the third section, the papers of which were afterwards transmitted for examination by the second.

The calculators of each section formed two divisions, each of which performed the whole work without communication with the other. The comparison of the results thus obtained afforded the verification.

By these means two manuscript copies of the whole of the table was formed, composed of seventeen volumes large folio, and containing,

1. An introduction, consisting of an exposition of the analytical formulæ, the use of the trigonometrical table, and a great number of particular and auxiliary tables.
2. The natural sines for each for 10,000th part of the quadrant, calculated to twenty-five places of decimals, with seven or eight columns of differences, to be published with twenty-two decimals, and five columns of differences.
3. The logarithms of the sines for each 100,000th of the quadrant, calculated to fourteen decimals with five columns of differences.
4. The logarithms of the ratios of the sines to the arcs for the first five thousand 100,000th part of the quadrant, calculated to fourteen decimals, with three columns of differences.
5. The logarithms of the tangents corresponding with the logarithm of the sines.
6. The logarithms of the ratios of the tangents to the arcs calculated like those of the fourth article.
7. Logarithms of numbers from 1 to 100,000, calculated to nineteen places of decimals.
8. The logarithms of 100,000 to 200,000, calculated to twenty-four decimals, in order to be published to twelve decimals, and three columns of differences.

This explanation shews how far superior this work is to the *Opus Palatinum de Triangulis*, began by Rheticus, the disciple of Copernicus, and completed in 1596 by Otho, the disciple of Rheticus, or to the *Thesaurus Mathematicus*, published by Pitiscus in 1613, and the last tables of Vlacq. This was the judgment of the Commissaries, which the Class of the Physical and Mathematical Societies of the National Institute nominated to give an account of

of this work. It is much to be wished, that the government would resume the impression of this immense work, which was suspended at the fall of the assignats.

To the notice, of which we have given an account, Cit. Prony has added a bibliographic anecdote respecting the *Opus Palatinum*, very interesting to those who seek rare books. They will learn with pleasure, that there are a few copies of this book, in which 86 pages have been reprinted, containing the co-tangents and co-secants of the seven first degrees, of which the last figures were uncertain in the first copies, and which Pitiscus could not correct, but by carrying the sines of these degrees to twenty-two decimals, which had been calculated only to fifteen.

This note is followed by the report of Citizens Lagrange, Laplace, and de Lambre, respecting these tables, in which we find various interesting details on the construction of trigonometrical tables in general, and the methods employed by one of the Commissaries to verify those which were submitted to them.

Bulletin de Science, No. 52, An 9.

VII.

Observations relative to the Means of increasing the Quantities of Heat obtained in the Combustion of Fuel. By COUNT RUMFORD.*

IT is a fact which has been long known, that clays, and several other incombustible substances, when mixed with sea coal, in certain proportions, cause the coal to give out more heat in its combustion, than it can be made to produce when it is burned pure or unmixed; but the cause of this increase of heat does not appear to have been yet investigated with that attention which so extraordinary and important a circumstance seems to demand.

Daily experience teaches us that all bodies,—those which are incombustible,—as well as those which are combustible, and actually burning, throw off in all directions heat, or rather calorific (heat-making) rays, which generate heat wherever they are stopped, or absorbed; but common observation was hardly sufficient to shew any perceptible difference between the quantities of calorific rays thrown off by different bodies, when heated to the same temperature, or exposed in the same fire; although the quantities so thrown off, might be, and probably are, very different.

It has lately been ascertained, that when the sides and back of an open chimney fire-place, in which coals are burned, are composed of fire-bricks, and heated red-hot, they throw off into the room incomparably more heat than all the coals that could possibly be put into the grate, even supposing them to burn with the greatest possible degree of bright-

* Journal of the Royal Institution, p. 28.

ness. Hence it appears that a red-hot burning coal does not send off near so many calorific rays as a piece of red-hot brick or stone, of the same form and dimensions; and this interesting discovery will enable us to make very important improvements in the construction of our fire-places, and also in the management of our fires.

The fuel instead of being employed to heat the room *directly*, or by the direct rays from the fire, should be so disposed, or placed, as to *heat the back and sides of the grate*; which must always be constructed of fire-brick, or fire-stone, and *never of iron, or of any other metal*. Few coals, therefore, when properly placed, make a much better fire than a larger quantity; and shallow grates, when they are constructed of proper materials, throw more heat into a room, and with a much less consumption of fuel, than deep grates; for a large mass of coals in the grate arrests the rays which proceed from the back and sides of the grate, and prevents their coming into the room; or, as fires are generally managed, it prevents the back and sides of the grate from ever being sufficiently heated to assist much in heating the room, even though they be constructed of good materials, and large quantities of coals be consumed in them.

It is possible, however, by a simple contrivance, to make a good and an economical fire in almost any grate, though it would always be advisable to construct fire-places on good principles, or to improve them by judicious alterations, rather than to depend on the use of additional inventions for correcting their defects.

To make a good fire in a bad grate, the bottom of the grate must be first covered with a single layer of balls, made of good fire-bricks, or artificial fire-stone, well burnt, each ball being perfectly globular, and about $2\frac{1}{2}$ or $2\frac{3}{4}$ inches in diameter. On this layer of balls the fire is to be kindled, and, in filling the grate, more balls are to be added with the coals that are laid on; care must, however, be taken in this operation to mix the coals and the balls well together, otherwise, if a number of the balls should get together in a heap, they will cool, not being kept red-hot by the combustion of the surrounding fuel, and the fire will appear dull in that part; but if no more than a due proportion of the balls are used, and if they are properly mixed with the coals, they will all, except it be those perhaps at the bottom of the grate, become red-hot, and the fire will not only be **very** beautiful, but it will send off a vast quantity of radiant heat into the room; and will continue to give out heat for a great length of time. It is the opinion of several persons who have for a considerable time practised this method of making their fires, that more than one third of the fuel usually consumed may be saved by this simple contrivance. It is very probable that, with careful and judicious management, the saving would amount to one half, or fifty per cent.

As these balls, made in moulds, and burnt in a kiln, would cost very little, and as a set of them would last a long time, probably several years, the saving of expence in heating rooms by chimney fires with bad grates, in this way, is obvious; but still, it should be remembered that a saving quite as great may be made by altering the grate, and making it a good fire-place.

In

In using these balls, care must be taken to prevent their accumulating at the bottom of the grate: as the coals go on to consume, the balls mixed with them will naturally settle down towards the bottom of the grate, and the tongs must be used occasionally to lift them up; and, as the fire grows low, it will be proper to remove a part of them, and not to replace them in the grate till more coals are introduced: a little experience will shew how a fire made in this manner can be managed to the greatest advantage, and with the least trouble.

Balls made of pieces of any kind of well burnt hard brick, though not equally durable with fire-brick, will answer very well, provided they be made perfectly round; but if they are not quite globular their flat sides will get together, and by obstructing the free passage of the air amongst them, and amongst the coals, will prevent the fire from burning clear and bright.

The best composition for making these balls, when they are formed in moulds, and afterwards dried and burnt in a kiln, is pounded crucibles mixed up with moistened Sturbridge clay; but good balls may be made with any very hard burnt common bricks, reduced to a coarse powder, and mixed with Sturbridge clay, or even with common clay. The balls should always be made so large as not to pass through between the front bars of a grate.

These balls have one advantage, which is peculiar to them, and which might perhaps recommend the use of them to the curious, even in fire-places constructed on the best principles; they cause the cinders to be consumed almost intirely; and even the very ashes may be burnt, or made to disappear, if care be taken to throw them repeatedly upon the fire when it burns with an intense heat. It is not difficult to account for this effect in a satisfactory manner, and in accounting for it we shall explain a circumstance on which it is probable that the great increase of the heat of an open fire, where these balls are used, may, in some measure, depend. The small particles of coal, and of cinder, which, in a common fire, fall through the bottom of the grate, and escape combustion, when these balls are used can hardly fail to fall and lodge on some of them; and, as they are intensely hot, these small bodies which alight upon them in their fall, are soon heated red hot, and disposed to take fire and burn; and, as fresh air from below the grate is continually making its way upwards amongst the balls, every circumstance is highly favourable to the rapid and complete combustion of these small inflammable bodies. But if these small pieces of coal and cinder should, in their fall, happen to alight upon the metallic bars which form the bottom of the grate; as these bars are conductors of heat, and, on account of that circumstance, as well as of their situation, *below* the fire, never can be made very hot, any small particle of fuel that happens to come into contact with them, not only cannot take fire, but would cease to burn, should it arrive in a state of actual combustion.

These facts are very important, and well deserving of the attention of those who may derive advantage from the improvement of fire places, and the economy of fuel.

There are some circumstances which strongly indicate that an admixture of incombustible bodies with fuel, and especially with coal, cause an increase of the heat, even when the fuel is burnt in a closed fire-place. No fire-place can well be contrived more completely closed than those of the iron stoves in common use in the Netherlands; but in these stoves, which are heated by coal fires, a large proportion of wet clay is always coarsely mixed with the coals before they are introduced into the fire-place. If this practice had not been found to be useful it would certainly never have obtained generally; nor would it have been continued, as it has been, for more than two hundred years.

The combination of different substances, combustible, and incombustible, to form, artificially, various kinds of cheap and pleasant fuel, particularly adapted for the different processes in which the fuel is employed, is a subject well worthy of the attention of enterprising and ingenious men. How much excellent fuel, for instance, might be made with proper additions, and proper management, of the mountains of refuse coal dust that lie useless at the mouths of coal pits; and how much would it contribute to cleanliness and elegance if the use of improved coke, or of hard and light fire balls, could be generally introduced in our houses and kitchens, instead of crude, black, powdery, dirty, sea coal. Of the great economy that would result from such a change there cannot be the smallest doubt.

It is a melancholy truth, but, at the same time, a most undisputable fact, that, while the industry and ingenuity of millions are employed, with unceasing activity, in inventing, improving, and varying those superfluities which wealth and luxury introduce into society, no attention whatever is paid to the improvement of those common necessities of life on which the subsistence of all, and the comforts and enjoyments of the great majority of mankind, absolutely depend.

Much will be done for the benefit of society, if means can be devised to call the attention of the active and benevolent to this long neglected, but most interesting, subject.

The Royal Institution seems to be well calculated to facilitate and expedite the accomplishment of this important object. Indeed it is more than probable that this, precisely, is the object which was principally had in view in the foundation and arrangement of that establishment.

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

Diseases of horned Cattle and Sheep.

W_H P. Whyte, Esq. of Worcester, has drawn the outline of a plan for ascertaining and defining the different diseases of horned cattle and sheep, their causes, and means of prevention or cure. He intends to publish a prospectus speedily; but in the mean time his outline, which is of some length, appears in that useful work "the Annals of Agriculture" The present proposals are as follow:

1. That

1. That a tour shall be made at different times through all the principal grazing districts in that part of Great Britain called England, similar to those of Arthur Young, Esq. or Mr. Marshall, the writer of the valuable volumes on the Rural Economy of the different Counties, for the purpose of ascertaining and defining the different diseases incident to horned cattle and sheep, the various causes whence they arise, and the means of preventing and remedying them.

2. That every particular disease (so far at least as such tour shall afford opportunity) shall undergo a scientific and full investigation, with a view to the different ends proposed.

3. That minutes of every material occurrence and transaction relative to the objects in view shall be regularly taken and preserved, with a view to future publication; in particular, that these shall record the nature of particular diseases, the districts where they are most prevalent, the real, apparent, or probable causes of them, the provincial opinions on the subject, the symptoms of such diseases, the means which have been hitherto employed with intent to remedy them, the success or otherwise of such means, the morbid anatomical appearances after death, the certain or probable means of prevention, &c.

4. That the result of the whole shall be published in octavo volumes, either in distinct parts once in every six or twelve months, or, arranged in systematical form, at the conclusion of the tour, as may be thought advisable by the Secretary to the Board and the Author, upon such terms as shall furnish them to the subscribers at a moderate expence.

5. That the Board shall afford their sanction to the undertaking by a letter of recommendation from the President; which the Tourist shall be at liberty to carry with him, and show occasionally.

6. That the expence of the undertaking and the remuneration to the Tourist shall be borne and defrayed by a public subscription and the money to arise from publication of the minutes.

7. That subscriptions shall be received (probably) by Arthur Young, Esq. F. R. S. Secretary to the Board.

8. That the Royal Family and Nobility of this kingdom, Foreign Potentates and Nobility, and Public Bodies and Societies established by royal charter, be invited to subscribe at pleasure.

9. That the Gentry, Land Owners, Breeders, Graziers, and all others shall become subscribers upon the payment of to the Receiver.

10. That a sufficient number of copies of the minutes shall be handsomely printed on a superfine royal quarto paper, and hot pressed, and delivered *gratis* to all subscribers of any sum, being five guineas more than the ordinary subscription.

11. That so soon as pounds are subscribed, the Tourist will proceed in the undertaking.

On the New Alkali called Pneum of Dr. Hahneman.

Messrs. Klaproth, Karstein, and Hermstadt having published in the Literary Gazette of Jenu, that the result of their examination of Mr. Hahneman's new alkali was, that it is merely the acid of borax supersaturated with soda, that is to common borax. The Doctor published his answer addressed to those chemists, as follows :

" I may be deceived, but I am not capable of deceiving. I was at work upon crude borax; pot-ash dropped into a solution not yet disposed to crystallize, afforded an abundant precipitate in the form of powder. But as authors assure us that borax becomes uncrySTALLIZABLE by the addition of alkali, it is not surprizing that I should have considered this precipitate as a new and particular substance. The re-agents also shewed phenomena different from those presented by common borax. But in order to clear it as I thought from all adherent borax, I saturated it exactly with acetic acid, and mixed the concentrated ley with equal parts of spirit of wine. After the separation of what remained insoluble, I supposed myself to be in possession of the alkaline part, combined with the acetic acid dissolved in the spirit of wine, and I separated this acid by adding one of the three alkalies. The salt thus precipitated and washed, appeared to me to possess a sufficient number of properties different from those of common borax, to be considered as a peculiar alkali. The general notion of chemists of an unsaturated excess of alkali in borax led me into error; for how could this excess exist in my borax of soda, since the mineral acids added to my borax to dryness escaped by heat? How could the excess of soda exist in my borax in a caustic state? This excess either is not soda, or it is not a real excess, it is adopted by chemists without foundation. This last is in fact the case. The acid of borax neutralizes the soda in such a manner, and extends its neutralizing power in such a manner on the part which has hitherto appeared to us to be in excess, that the carbonic acid and the mineral acids scarcely enter into any intimate combination with this excess."

Journal de Physique, Messidor An. 9.

Facts and Discoveries respecting Galvanism.

In a letter from Berlin inserted in the Magazine Encyclopedique I. 1801. pag. 110, dated April last, several experiments are related by Helvige, a Swedish major, Bourguet, professor of chemistry, Hermann, professor of natural philosophy, and Dr. Grapengieffer. These philosophers repeated the experiments made by Volta, together with those which we added in this country. Their pile consisted of gold, and zinc with cloth moistened with salt water. Other metals were used, namely, silver and copper, instead of the gold, without affording any determinate ground for preference, as it appeared to these operators. They found that the galvanic spark is obtained with much greater facility, when one of the two conductors terminates in very fine pointed iron wire, and the other in a knob or button. This discovery presented itself when the gold leaf of the electrometer of Bennet

was

was made part of the circuit; and since making that observation they constantly armed the extremities of their conductors with gold leaf; by which means they obtained with ease a strong spark, and used the same method with the greatest success in setting fire to phosphorus, sublimed sulphur, fulminating gas, and gunpowder, the latter of which is not easily fired by the electric fluid. It was remarked, that in those experiments the portion of gold leaf which covered the extremity of the conductors, was itself fused and reduced into a globule more or less perfect, according to the force of the spark.

They repeated my experiment with the condenser, and found agreeably to what I had observed, that the silver is in the negative state, and the zinc positive. By the intervention of the condenser they charged a jar, but could not obtain the slightest galvanic commotion. But by means of two condensers (by the process of doubling I suppose) they repeated upon the Electrophore the experiment of producing the figures of Lichtenberg, with the same varieties and forms which would have been produced by the electric fluid.

Dr. Grapengeter has applied the galvanic energy in the course of medical practice, and observes, that the similitude of this action with that of the electric fluid cannot be mistaken; but these philosophers think, that a great dissimilitude, or variation in these two powers, was indicated by an experiment they made of insulating the whole apparatus, together with the tube in which the process of decomposing water was going forward. In this situation they electrified the whole apparatus by the common machine, and obtained very considerable sparks, without producing any change in the phenomena between the wires in the tube. Hence they think that those powers may be imagined to be independent of each other. Like many philosophers in this country, as well as elsewhere, they are disposed to think that the galvanic power depends upon the transition of oxygen; but it may be observed, that there are many unequivocal facts, which seem to prove that the common electricity also is never excited, but by the condensation or development of the same principle.

Galvanic Flash.

Mr. Cruickshank has favoured me with the remark, that a mistake appears in the use of the words zinc-face and silver face, at page 240 of our Journal for September, where an account is given of the luminous signs of galvanism corresponding with the two electricities. I here relate the object in his own words: *the brush or flash always appears at the extremity of the wire, which if immersed in water would disengage hydrogen, or what has been generally called the silver wire, whereas the zinc, or oxygen wire, exhibit only a faint luminous point.* And as he perfectly agrees in the remarks made at the page last quoted, respecting the plus and minus electricities, he is disposed to suspect, that there may be some mistake in the original experiments, which were made to ascertain the state as to plus and minus of the extremities of the pile. I will repeat those experiments, in order that the facts may clear up this matter, and shew whether there be any striking difference between the galvanic and electric lights.

Effect

*Effect of Galvanism in a Case of Paralysis of the Muscles of the Left Side of the Face.**By CITIZEN HALLE*.*

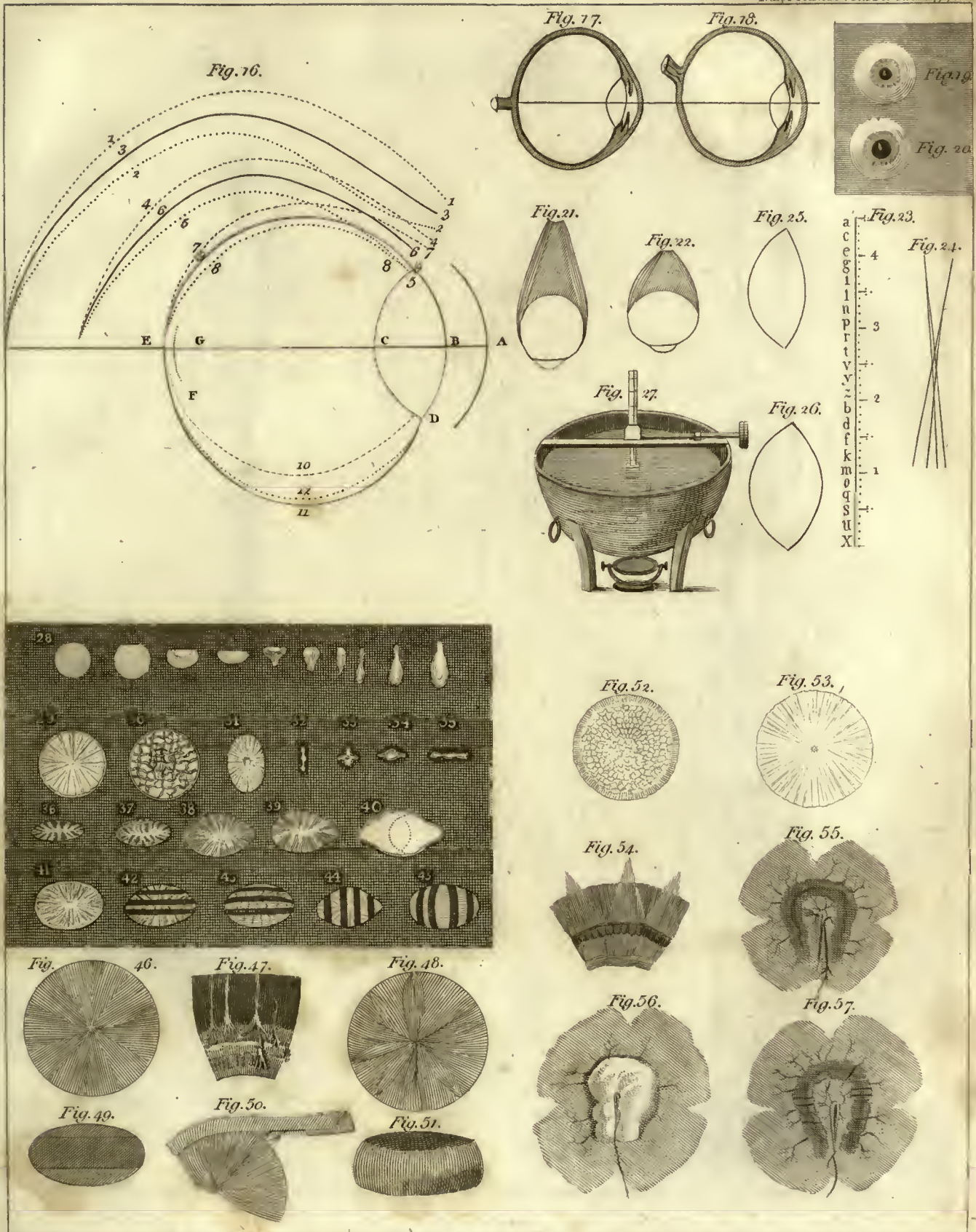
A man whose muscles of the left side of the face, and the interior muscles of the globe of the eye on the same side were paralysed in consequence of a cold, had been electrified several times. He experienced no sensation nor contraction when the affected part received the spark, and there was scarcely the slightest contraction in the zygomaticus major when the electric shock was applied. This man was subjected to the galvanic action of a pile of fifty sets, by forming a communication between the different points of the diseased side of the face, and the two extremities of the pile by means of metallic excitators.

At the moment of contact all the muscles of the face became contracted. The patient felt pain with a very disagreeable sensation of heat, the eye became convulsed, tears flowed involuntarily, and redness with swelling was perceived on the different parts which were touched.

These experiments, which appear to afford some means of comparing the effects of galvanism with electricity, were repeated several successive days at the school of medicine, and on the 26th of Prarial Citizen Halle gave an account of the same to the National Institute. He observed, that the muscles remained contracted for some minutes after the galvanic commotion, and that the left eye even followed the motion of the right.

In this application of galvanism to the human body, Citizen Halle observed some very singular anomalies. The pile was often very long before it communicated its effect; sometimes it ceased to produce any effect for several seconds together. In both these cases it seemed as if the fluid met some obstacle in its course. For in these circumstances it was necessary to moisten the skin, to rub it, and to change the relative position of the rings, in order to make the communication. In general it was observed, that in order that the sensation should be speedy, it was not enough that the skin should be wetted, but that it must be, as it were, soaked and softened with the water. He himself, as well as several other persons, made trial of the sensation which the galvanism produces. It resembles in some respects that of the action of several needles forced at the same time into the skin. It is a pungent pain, accompanied with a sense of heat and a slight metallic taste, when the exciters were applied near the salivary glands.

* Communicated to the National Institute of France, and inserted in the Bulletin des Sciences, No. 51, An. IX.

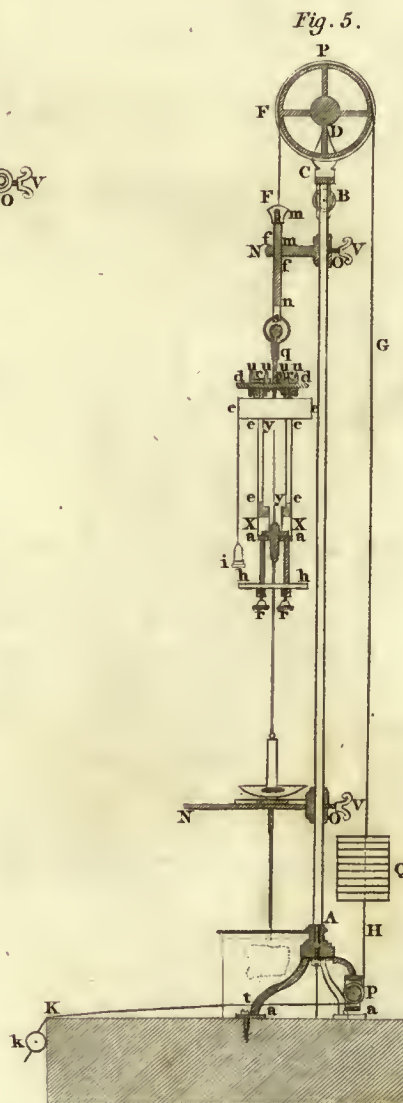
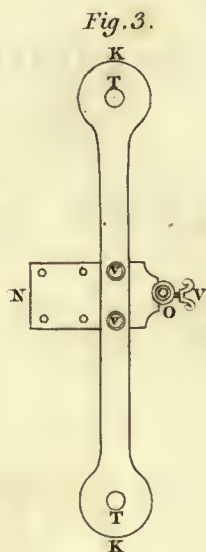
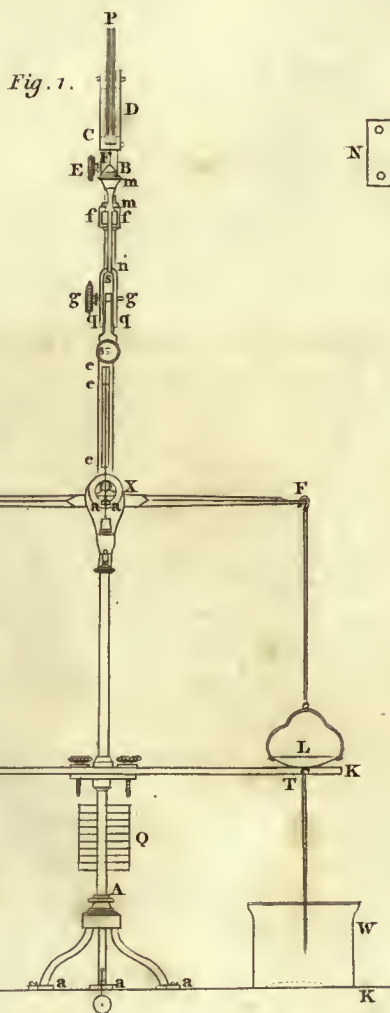
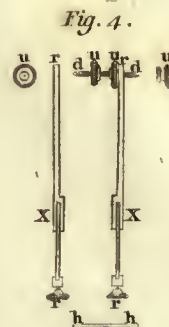
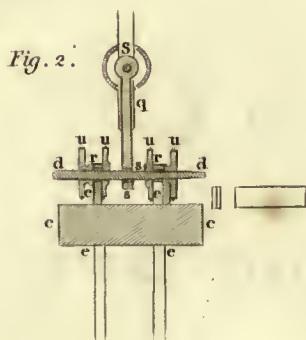




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A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

DECEMBER, 1801.

ARTICLE I.

*Observations on the Basaltic Coast of Ireland. In a Letter from WM. RICHARDSON, D.D.
to the Honourable GEO. KNOX, M. P. Communicated by Mr. Knox.*

DEAR SIR,

WHEN you left Portrush in August last, you lamented that you could not accompany me on two expeditions I was then preparing for, one to explore our coast in search of the WHYNN DYKES (we call them GAWS) which issuing from it at right angles proceed nearly due northward, and cross most of the Hebrides, cutting through all substances indifferently.

I found four at BENGORE HEAD, in addition to the two at the Giant's Causeway we examined together, and whose internal prismatic construction, and other circumstances astonished you so much; I found also four at FAIRHEAD, making ten in all, every one of which that I could get access to, differed in some material points from all the others.

Mr. Mills (Phil. Transf. 1790,) mentions several of these WHYNN DYKES he met with in the Scotch islands, but he does not seem to have examined them with attention, or noticed their differences, if they really differ from each other as ours do.

As soon as I am able to explore the coast from DUNSCURICH to BALLINTAY, where possibly I may find more, I will (according to my promise) give you an accurate account

of these wonderful walls, which seem to me utterly irreconcilable to any of the theories invented by cosmogonists.

In this Letter I will confine myself to the mighty promontory of FAIRHEAD, which forms the north-east point of our island, and a line drawn from it to Cape Clear in Kerry measures its greatest dimension.

Before I proceed to particulars, I must dwell a little on its magnificence, both because you yourself, when admiring from the water the vast basaltic precipices of BENGORE, (370 feet high) seemed to doubt me, when I told you that at FAIRHEAD they were far more stupendous; and also because Mr. PENNANT asserts, that the basaltic pillars at STAFFA *far exceed the Irish in grandeur*.

It has of late been much the fashion to prefer STAFFA to any thing our basaltic coast can boast of, with what foundation will soon appear.

In point of grandeur, mensuration affords a good test, and fortunately we have the heights of the pillars and precipices at STAFFA accurately taken by Sir Joseph Banks, and I make myself answerable for the measures I state at FAIRHEAD.

In Staffa the longest pillar or range of pillars is 55 feet; and the highest point of the precipice and island 126; (this last assertion is from recollection) whereas at Fairhead the height of the face of the columnar precipice, that is, the height of each pillar in a range of near an English mile, varies only from 252 to 250 feet, and from the angle (see view of Fairhead taken at four miles distance W. S. W.) to the level of the sea, the perpendicular height of the precipitous declivity is at least 300 feet, making together the height of the promontory 550 feet.

After comparing these measures, it is mortifying to observe the animation with which Sir Joseph Banks describes the magnificence of Staffa, while Fairhead remains unnoticed.

I apprehend it is by far the most superb colonade of basaltic pillars yet discovered, though Mr. KIRWAN, on the authority of Mr. Charpentier says, the pillars at STOLPE are 300 feet high, but the Encyclopedie tells us, these same pillars at STOLPE are but 17 or 18, and Gentlemen (particularly my friend Archdeacon Lovel) who have visited STOLPE, assure me that the latter dimensions seem to them the true ones.

The vast perpendicular precipice which lines the upper part of Fairhead promontory, is twice cut into by very acute angles; the westernmost of these is as it were bisected by a mighty wall, composed of pillars near 150 feet long each, while the easternmost affords a path by which I descended to the bases of the pillars; as I stood at the opening of this angle the scene about me was the most stupendous I ever beheld; on my right and left hand enormous walls reaching almost to the sky, intirely columnar, the diameter of each pillar exceeding two feet, but quite out of proportion to their enormous height.

The view below me was terrific, for I stood on the top of a second stratum of perpendicular pillars, probably 80 feet long each, with the declivity from the bottom of these pillars to the sea, steep and precipitous, not much less than 300 feet.

The

The views from the summit of this promontory are highly gratifying to those who delight in the sublime, but it is by coasting its base alone that the naturalist will obtain entertainment or information; this however is an expedition of difficulty and uncertainty, from the rapidity of the tide that runs close to this headland, and the violent surf which generally beats against its base; twice I reached the north-west point of this formidable head without daring to proceed, the third time I was more fortunate, and amply repaid for my perseverance.

While at Fairhead nature seems to have exerted her utmost powers in displaying a magnificence unrivalled any where else, at the same time she appears in the construction of this promontory to have adopted different principles from those she has hitherto been governed by in the formation of the rest of our basaltic country.

I had always observed that our strata were in the same place, all steadily parallel to each other, and I shewed you that this rule obtained in all the precipices we rowed along together; but at Fairhead the case is different, for there the columnar strata are inclined to each other in a considerable angle.

I also shewed you, that in every separate stratum the material was uniform through its whole extent, the principle of construction, and the degree of perfection with which it was executed, the same.

At Fairhead on the contrary, the material is sensibly changed, in passing from the west to the east side, the perfection in the workmanship of the pillars considerably abated, and a new principle of internal construction introduced.

A most extraordinary circumstance also occurs here, which I have not met with in any other of the columnar groups with which our basaltic country abounds, nor do I find any thing similar mentioned by the writers on these subjects.

In viewing Fairhead from any distance, many of its pillars appear to be of a most enormous size, but upon a near approach those gigantic columns from ten to twenty feet diameter, are found to be formed by an accumulation of the smaller ones (from two to three feet) which compose the promontory, and adhere together with such force, that when the mighty pillar tumbles down (as sometimes happens) it breaks in pieces by the violence of a fall 250 feet perpendicular, without separating from each other, the smaller pillars of which it is composed.

The steep declivity is covered quite down to the water edge with enormous fragments, or frusta of these pillars, they are always cylindrical, and to look at their sides they resemble an assemblage of great beams of timber consolidated together, while their bases (generally planes, and at right angles with their axes) exhibit a tessellated pavement, exactly like the surface of the Giant's Causeway, but the polygons are somewhat larger, and approach more to equilateral figures.

Sometimes these smaller pillars are continued quite up to the top, of their original size, at other times when half way up they merge into, and form greater pillars, which are continued to the summit, at other times again they merge into the solid rock.

It also frequently happens that the lower half of columns, both great and small, has fallen down, leaving the upper half apparently unsupported, yet firm in its place; this last circumstance, however, is common in all our basaltic precipices.

This suspension of the upper parts of pillars, and still more the adhesion of their sides, which I have just now shewn to be so strong as not to yield to the shock of a mighty fall, prove, that at least in this place there are no intervals between the pillars.

From analogy I have long suspected this to be the case, every variety of our basalt is of prismatic construction, and in *all*, the prisms adhere so closely together as to be separable by the crow or sledge alone, the columnar is the only one that has intervals between its prisms, and these I have little doubt proceed from external causes, but it is impossible to penetrate so far into a range of pillars as to bring the point to certainty; it is by catching at accidental circumstances that we are enabled to throw any light upon it.

As I coasted along the foot of the promontory, the magnificence of the scene above me baffled all description, the mighty wall which struck me so forcibly when on the top of the cliff, was now an insignificant object, and the angle through which I had descended to the foot of the precipice, was diminished almost to nothing.

A little to the eastward of this angle (called the Grey Man's Bath) we came opposite to the place where a considerable part of the precipice had fallen down about 25 years ago; its length seemed to be near a furlong, and its depth into the face of the rock must have exceeded 150 feet; the materials of this immense mass are scattered in vast fragments piled upon one another from the foot of the perpendicular precipice, quite down to the water edge, forming by their accumulations a wild variety of caverns, arches, and passages beneath the ruins.

The face of rocks which is now laid bare, and which before the disruption was far in the interior, is by no means so columnar as the rest of the surface; sometimes the vast pillars alone are visible, sometimes the rocks seem quite plain, and in one place the lines separating three or four pillars after ascending about 150 feet, faintly marked, make a slight curve for a few feet, and then merge in the solid rock.

All this looks as if the columnar construction did not reach far into the interior of our precipices; but exclusive of the unfairness of drawing a general conclusion from this solitary instance, I must admit I could not get so near the base of the perpendicular face, as to make any accurate observations; besides, the material seems to undergo a considerable change in the passage from the west to the east side of Fairhead, to have assumed a darker colour, and from many of the fragments looks as if composed of small trapezoidal prisms, like the Gaw, or Whynn Dyke we examined together at PORT SPAGNA BENGORE, the internal construction of whose massive horizontal prisms was disclosed to us by the nervous arm, and weighty sledge of your friend Mr. Crampton, without whose powerful assistance we ourselves would in vain have attempted to discover it.

I have not met with any writer who notices this curious circumstance attending many of our basalts, to wit, that exclusive of their external prismatic form, they have, as it were, a

subordinate

subordinate internal principle of construction, the great prisms breaking into smaller; columnar basalt has rarely this property, I have found it only in the pillars I shewed you at Ballylagan) and at the east side of Fairhead, but all the gaws or dykes seem to be constructed upon this principle, and the two strata which alternate so frequently at Portrush possess the same property in an high degree, and one of these you and your friend Mr. Chenevix have proved by analysing it, that it is pure basalt.

To return to Fairhead, immediately below the Grey Man's Bath, a new stratum of columnar basalt begins to appear, and contrary to what I have observed every where else, not parallel to the one above it (which seems nearly horizontal) but dipping to the south-east, of course as we advance in this direction these strata diverge from each other, and the increasing space between them is filled up with new materials, to wit, alternate strata of free stone and coal.

These strata are all parallel to the lower columnar stratum; the one immediately resting upon it is coal, this is the vein that was last wrought at MURTOGH, and the adit to the mine runs in along the heads of the basalt pillars.

On tracing these alternate strata in a western direction, they are found to terminate successively against the bases of the upper columnar stratum, always making with it an angle equal to that of the inclination of the two great strata.

Here we have probably the strongest instances that have yet occurred any where of the contacts of coal with columnar basalt, for while the lowest stratum of coal rests with its whole surface on the heads of basalt pillars, every stratum of coal above it comes by regular succession into contact with the bases of the pillars of the upper columnar stratum. I must confess I did not examine these contacts, the acclivity was so steep, and so strewed with enormous masses of rock as to be inaccessible at least to me.

You must observe that the strata at FAIRHEAD totally differ both in materials and arrangement, from the strata composing BENGORE HEAD, which we counted together, and found to be 16 in number; in truth no two of our vast precipices, whether hanging over the sea, or in the interior of the country, perfectly (or I may say nearly) resemble each other in these circumstances.

This variety in the stratification of our basalt country, must be a subject of much curiosity, particularly to geologists, and was no doubt considered as such by Mr. WHITEHURST and MILLS, when they promised in their title pages an account of our strata, yet in the body of their works forgot the topic.

If you can find leisure for another excursion so far north next summer, our strata will afford us an ample fund of entertainment, both in trying the theories of cosmogonists by the test of these strata, and in conjecturing from their interruptions and other circumstances, what changes our globe has undergone since its creation.

I am, SIR,

Your very humble Servant,

WILLIAM RICHARDSON, D. D.

Late Fellow of Trinity College, Dublin.

Observations

Clonsilla House, Moy, Oct. 22, 1801.

II.

Observations on a Mist which prevailed at Maestricht on the 14th Nivose, in the Year 8, by CIT. PAISSE', with Remarks by CIT. PARMENTIER.*

IT is of great importance in the accurate sciences to pay attention to all the circumstances upon which observations can be founded. If this part of natural philosophy were cultivated with the care it deserves, there is no doubt but we should be much more intimately acquainted with the influence of meteors on the health of men, which it so evidently affects. Those fogs which are almost periodically renewed, present to the ministers of health an extensive field of observation, from which it should appear, that the most useful consequences respecting the origin of diseases may be deduced.

The mist which forms the subject of the present communication was so peculiar in its appearance and effects, that I have concluded that it might be of advantage to give a detailed account of the same.

On the morning of the 11th Nivose Reaumur's thermometer had descended to $13\frac{1}{2}^{\circ}$ below O, where it remained for most part of the day, with no greater variation than $\frac{1}{2}$ a degree. At seven in the evening it was constant at the same temperature, and the following day at seven in the morning it had risen two degrees above the freezing point, and at eleven o'clock the thaw had unequivocally taken place at the temperature of 5 degrees. At two in the evening the thermometer marked $6\frac{1}{2}$ degrees, the thaw continuing; about five in the evening a slight mist appeared, which continued till half past seven; at eight it was intirely dissipated, and the moon shone bright, at which time the thermometer descended again within four degrees of the freezing point; at nine its elevation did not exceed one degree, when the snow began to harden. On the 13th at seven in the morning the thermometer stood at seven degrees above O; at nine, eight degrees, and at noon it rose to the ninth degree, and the sun which appeared from time to time in the course of the day had considerable force. The thermometer kept at the same elevation, and the thaw continued its progress at eight in the evening.

On the 14th day of the month the horizon was covered with a mist of inconsiderable density. It appeared like an evaporation from the earth arising about a metre above the surface. At nine it rose to the height of several metres without becoming less dense; at eleven it became more considerable; and at noon it was so thick that two persons at a very small distance asunder could not see each other without much difficulty. At three in the evening its density was still greater, and its smell so fetid as strongly to impede respiration. The thermometer during this time underwent no greater change than half a degree, not passing beyond the ninth degree. The smell and acrid effects of this meteor were so dis-

† Annales de Chimie, XXXIII. 217.

agreeable, that I thought for a time that this property was communicated by the vapours of the bituminous combustible which is used in this country, and I did not give up my opinion till I had found myself as strongly affected with it in the open country as in the town. My eyes were irritated so as to flow with involuntary tears. I returned home after experiencing the greatest difficulties, and observing that the fog in the streets was much more elevated than in the open country. I observed, as Citizen Fourcroy did on a former occasion at Paris, that it was more easy to find the way in the narrow streets, than in such as were wider, in the former of which the fog descended with much less rapidity.

III.

On the Mechanism of the Eye. By THOMAS YOUNG, M.D. F.R.S.

(Continued from page 303.)

HITHERTO I have endeavoured to shew the inconveniences attending other suppositions, and to remove the objections to the opinion of an internal change of the figure of the lens. I shall now state two experiments, which, in the first place, come very near to a mathematical demonstration of the existence of such a change, and, in the second, explain in great measure its origin, and the manner in which it is effected.

I have already described the appearance of the imperfect image of a minute point at different distances from the eye, in a state of relaxation. For the present purpose, I will only repeat, that if the point is beyond the furthest focal distance of the eye, it assumes that appearance which is generally described by the name of a star, the central part being considerably the brightest. (Plate XV. Fig. 36—39.) But, when the focal distance of the eye is shortened, the imperfect image is of course enlarged; and, besides the necessary consequence, the light is also very differently distributed; the central part becomes faint, and the margin strongly illuminated, so as to have almost the appearance of an oval ring. (Fig. 41.) If I apply the slider of the optometer, the shadows of the slits, while the eye is relaxed, are perfectly straight, dividing the oval either way into parallel segments: (Fig. 42, 44.) but, when the accommodation takes place, they immediately become curved, and the more so the further they are from the centre of the image, to which their concavity is directed. (Fig. 43, 45.) If the point be brought much within the focal distance, the change of the eye will increase the illumination of the centre, at the expense of the margin. The same appearances are equally observable, when the effect of the cornea is removed by immersion in water; and the only imaginable way of accounting for the diversity, is to suppose the central parts of the lens to acquire a greater degree of curvature than the marginal parts. If the refraction of the lens remained the same, it is absolutely impossible that any change of the distance of the retina should produce a curvature in those shadows, which, in the

relaxed state of the eye, are found to be in all parts straight; and, that neither the form nor the relative situation of the cornea is concerned, appears from the application of water already mentioned.

The truth of this explanation is fully confirmed by the optometer. When I look through four narrow slits, without exertion, the lines always appear to meet in one point: but, when I make the intersection approach me, the two outer lines meet considerably beyond the inner ones, and the two lines of the same side cross each other at a still greater distance. (Plate XV. Fig. 24.)

The experiment will not succeed with every eye; nor can it be expected that such an imperfection should be universal: but one case is sufficient to establish the argument, even if no other were found. I do not however doubt, that in those who have a large pupil, the aberration may be very frequently observable. In Dr. Wollaston's eye, the diversity of appearance is imperceptible; but Mr. König described the intersection exactly as they appear to me, although he had received no hint of what I had observed. The lateral refraction is the most easily ascertained, by substituting for the slits a tapering piece of card, so as to cover all the central parts of the pupil, and thus determining the nearest crossing of the shadows transmitted through the marginal parts only. When the furthest intersection was at 38, I could bring it to 22 parts with two narrow slits; but with the tapered card only to 29. From these data we may determine pretty nearly, into what form the lens must be changed, supposing both the surfaces to undergo proportional alterations of curvature, and taking for granted the dimensions already laid down: for, from the lateral aberration thus given, we may find (by Prop. III.) the subtangents at about one-tenth of an inch from the axis; and the radius of curvature at each vertex, is already determined to be about 21 and 15 hundredths of an inch. Hence the anterior surface must be a portion of a hyperboloid, of which the greater axis is about 50; and the posterior surface will be nearly parabolical. In this manner the change will be effected, without any diminution of the transverse diameter of the lens. The elongation of its axis will not exceed the fiftieth of an inch; and, on the supposition with which we set out, the protrusion will be chiefly at the posterior vertex. The form of the lens thus will be nearly that of Plate XV. Fig. 26; the relaxed state being nearly as represented in Fig. 25. Should, however, the rigidity of the internal parts, or any other considerations, render it convenient to suppose the anterior surface more changed, it would still have room, without interfering with the uvea; or it might even force the uvea a little forwards, without any visible alteration of the external appearance of the eye.

From this investigation of the change of the figure of the lens, it appears that the action which I formerly attributed to the external coats, cannot afford an explanation of the phenomenon. The necessary effect of such an action would be, to produce a figure approaching to that of an oblate spheroid; and, to say nothing of the inconvenience attending a diminution of the diameter of the lens, the lateral refraction would be much more increased than the central; nor would the slight change of density, at an equal distance from the axis,

axis, be at all equivalent to the increase of curvature: we must therefore suppose some different mode of action in the power producing the change. Now, whether we call the lens a muscle or not, it seems demonstrable, that such a change of figure takes place as can be produced by no external cause; and we may at least illustrate it by a comparison with the usual action of muscular fibres. A muscle never contracts, without at the same time swelling laterally, and it is of no consequence which of the effects we consider as primary. I was induced, by an occasional opacity, to give the name of membranous tendons to the radiations from the centre of the lens; but, on a more accurate examination, nothing really analogous to tendon can be discovered. And, if it were supposed that the parts next the axis were throughout of a tendinous, and therefore unchangeable nature, the contraction must be principally effected by the lateral parts of the fibres; so that the coats would become thicker towards the margin, by their contraction, while the general alteration of form would require them to be thinner; and there would be a contrariety in the actions of the various parts. But, if we compare the central parts of each surface to the belly of the muscle, there is no difficulty in conceiving their thickness to be immediately increased, and to produce an immediate elongation of the axis, and an increase of the central curvature; while the lateral parts co-operate more or less, according to their distance from the centre, and in different individuals in somewhat different proportions. On this supposition, we have no longer any difficulty in attributing a power of change to the crystalline of fishes. M. Petit, in a great number of observations, uniformly found the lens of fishes more or less flattened: but, even if it were not, a slight extension of the lateral part of the superficial fibres would allow those softer coats to become thicker at each vertex, and to form the whole lens into a spheroid somewhat oblong; and here, the lens being the only agent in refraction, a less alteration than in other animals would be sufficient. It is also worthy of inquiry, whether the state of contraction may not immediately add to the refractive power. According to the old experiment, by which Dr. Goddard attempted to show that muscles become more dense as they contract, such an effect might naturally be expected. That experiment is, however, very indecisive, and the opinion is indeed generally exploded, but perhaps too hastily; and whoever shall ascertain the existence or non-existence of such a condensation, will render essential service to physiology in general.

Dr. Pemberton, in the year 1719, first systematically discussed the opinion of the muscularity of the crystalline lens*. He referred to Leeuwenhoek's microscopical observations; but he so overwhelmed his subject with intricate calculations, that few have attempted to develop it: and he grounded the whole on an experiment borrowed from Barrow, which with me has totally failed; and I cannot but agree with Dr. Olbers in the remark, that it is easier to confute him than to understand him. He argued for a partial change of the figure of the lens; and perhaps the opinion was more just than the reasons

* De Facultate Oculi qua ad diversas Rerum distantias se accommodat. L. B. 1719. Ap. Hall. Disp. Anat. IV. p. 301.

adduced for its support. Lobé, or rather Albinus*, decidedly favours a similar theory; and suggests the analogy of the lens to the muscular parts of pellucid animals, in which even the best microscopes can discover no fibres. Camper also mentions the hypothesis with considerable approbation†. Professor Reil published, in 1793, a Dissertation on the Structure of the Lens; and, in a subsequent paper, annexed to the translation of my former Essay in Professor Gren's Journal‡, he discussed the question of its muscularity. I regret that I have not now an opportunity of referring to this publication; but I do not recollect that Professor Reil's objections are different from those which I have already noticed.

Considering the sympathy of the crystalline lens with the uvea, and the delicate nature of the change of its figure, there is little reason to expect that any artificial stimulus would be more successful in exciting a contractive action in the lens, than it has hitherto been in the uvea; much less would that contraction be visible without art. Soon after Mr. Hunter's death, I pursued the experiment which he had suggested, for ascertaining how far such a contraction might be observable. My apparatus (Plate XV. Fig. 27.) was executed by Mr. Jones. It consisted of a wooden vessel blacked within, which was to be filled with cool, and then with warmer water: a plane speculum was placed under it; a perforation in the bottom was filled with a plate of glass; proper rings were fixed for the reception of the lens, or of the whole eye, and also wires for transmitting electricity: above these, a piece of ground and painted glass, for receiving the image, was supported by a bracket, which moved by a pivot, in connection with a scale divided into fiftieths of an inch. With this apparatus I made some experiments, assisted by Mr. Wilkinson, whose residence was near a slaughter-house: but we could obtain, by this method, no satisfactory evidence of the change; nor was our expectation much disappointed. I understand also, that another member of this Society was equally unsuccessful, in attempting to produce a conspicuous change in the lens by electricity.

XI. In man and in the most common quadrupeds, the structure of the lens is nearly similar. The number of radiations is of little consequence; but I find that in the human crystalline there are ten on each side, (Plate XV. Fig. 46.) not three, as I once, from a hasty observation, concluded§. Those who find any difficulty in discovering the fibres, must have a sight very ill adapted to microscopical researches. I have laboured with the most obstinate perseverance to trace nerves into the lens, and I have sometimes imagined that I had succeeded; but I cannot positively go further than to state my full conviction of their existence, and of the precipitancy of those who have absolutely denied it. The long nerves, which are very conspicuous between the choroid and sclerotic coats, divide each into two,

* De quibusdam Oculi Partibus, L. B. 1746. Ap. Hall. Disp. Anat. IV. p. 301.

† De Oculo Humano. L. B. 1742. Ap. Hall. Disp. Anat. VII. 2. p. 108, 109.

‡ 1794. p. 352, 354.

§ De Corp. Hum. Vir. Conf. p. 69.

three, or more branches, at the spot where the ciliary zone begins, and seem indeed to furnish the choroid with some fine filaments at the same place. The branches often reunite, with a slight protuberance, that scarcely deserves the name of a ganglion: here they are tied down, and mixed with the hard whitish-brown membrane that covers the compact spongy substance, in which the vessels of the ciliary processes anastomose and subdivide. (Plate XV. Fig. 47.) The quantity of the nerves which proceeds to the iris, appears to be considerably smaller than that which arrives at the place of division: hence there can be little doubt that the division is calculated to supply the lens with some minute branches; and it is not improbable, from the appearance of the parts, that some fibres may pass to the cornea; although it might more naturally be expected, that the tunica conjunctiva would be supplied from without. But the subdivisions which probably pass to the lens, enter immediately into a mixture of ligamentous substance, and of a tough brownish membrane; and I have not hitherto been able to develope them. Perhaps animals may be found in which this substance is of a different nature; and I do not despair that, with the assistance of injections, for more readily distinguishing the blood vessels, it may still be possible to trace them in quadrupeds. Our inability to discover them, is scarcely an argument against their existence: they must naturally be delicate and transparent; and we have an instance, in the cornea, of considerable sensibility, where no nerve has yet been traced. The capsule adheres to the ciliary substance, and the lens to the capsule, principally in two or three points; but I confess, I have not been able to observe that these points are exactly opposite to the trunks of nerves; so that, probably, the adhesion is chiefly caused by those vessels, which are sometimes seen passing to the capsule in injected eyes. We may, however, discover ramifications from some of these points, upon and within the substance of the lens, (Plate XV. Fig. 48.) generally following a direction near to that of the fibres, and sometimes proceeding from a point opposite to one of the radiating lines of the same surface. But the principal vessels of the lens appear to be derived from the central artery, by two or three branches at some little distance from the posterior vertex; which I conceive to be the cause of the frequent adhesion of a portion of a cataract to the capsule, about this point: they follow nearly the course of the radiations, and then of the fibres; but there is often a superficial subdivision of one of the radii, at the spot where one of them enters. The vessels coming from the choroid appear principally to supply a substance, hitherto unobserved, which fills up the marginal part of the capsule of the crystalline, in the form of a thin zone, and makes a slight elevation, visible even through the capsule, (Fig. 49—51.) It consists of coarser fibres than the lens, but in a direction nearly similar; they are often intermixed with small globules. In some animals, the margin of the zone is crenated, especially behind, where it is shorter: this is observable in the partridge; and, in the same bird, the whole surface of the lens is seen to be covered with points, or rather globules, arranged in regular lines, (Plate XV. Fig. 52.) so as to have somewhat the appearance of a honeycomb, but towards the vertex less uniformly disposed. This regularity is a sufficient proof that there could be no optical deception in the appear-

ance; although it requires a good microscope to discover it distinctly: but the zone may be easily peeled off under water, and hardened in spirits. Its use is uncertain; but it may possibly secrete the liquid of the crystalline; and it as much deserves the name of a gland, as the greater part of the substances usually so denominated. In peeling it off, I have very distinctly observed ramifications, which were passing through it into the lens; (Plate XV. Fig. 50.) and indeed it is not at all difficult to detect the vessels connecting the margin of the lens with its capsule; and it is surprising that M. Petit should have doubted of their existence. I have not yet clearly discerned this crystalline gland in the human eye; but I infer the existence of something similar to the globules, from the spotted appearance of the image of a lucid point already mentioned; for which I can no otherwise account, than by attributing it to a derangement of these particles, produced by the external force, and to an unequal impression made by them on the surface of the lens.

In birds and in fishes, the fibres of the crystalline radiate equally, becoming finer as they approach the vertex, till they are lost in a uniform substance, of the same degree of firmness, which appears to be perforated in the centre by a blood vessel. (Plate XV. Fig. 53.) In quadrupeds, the fibres at their angular meeting are certainly not continued, as Leeuwenhoek imagined, across the line of division; but there does not appear to be any dissimilar substance interposed between them, except that very minute trunks of vessels often mark that line. But, since the whole mass of the lens, as far as it is moveable, is probably endued with a power of changing its figure, there is no need of any strength of union, or place of attachment, for the fibres, since the motion meets with little or no resistance. Every common muscle, as soon as its contraction ceases, returns to its natural form, even without the assistance of an antagonist; and the lens itself, when taken out of the eye, in its capsule, has elasticity enough to re-assume its proper figure, on the removal of a force that has compressed it. The capsule is highly elastic; and, since it is laterally fixed to the ciliary zone, it must co-operate in restoring the lens to its flattest form. If it be inquired, why the lens is not capable of becoming less convex, as well as more so, it may be answered, that the lateral parts have probably little contractive power; and, if they had more, they would have no room to increase the size of the disc, which they must do, in order to shorten the axis; and the parts about the axis have no fibres so arranged as to shorten it by their own contraction.

I consider myself as being partly repaid for the labour lost in search of the nerves of the lens, by having acquired a more accurate conception of the nature and situation of the ciliary substance. It had already been observed, that in the hare and in the wolf, the ciliary processes are not attached to the capsule of the lens; and if by the ciliary processes we understand those filaments which are seen detached after tearing away the capsule, and consist of ramifying vessels, the observation is equally true of the common quadrupeds, and I will venture to say, of the human eye*. Perhaps this remark has been made by others,

* Vid. Hall. *Physiol.* V. p. 432. et Duverney, *ibi* citat.

but the circumstance is not generally understood. It is so difficult to obtain a distinct view of these bodies, undisturbed, that I am partly indebted to accident, for having been undeceived respecting them: but, having once made the observation, I have learnt to shew it in an unquestionable manner. I remove the posterior hemisphere of the sclerotica, or somewhat more, and also as much as possible of the vitreous humour, introduce the point of a pair of scissors into the capsule, turn out the lens, and cut off the greater part of the posterior portion of the capsule, and of the rest of the vitreous humour. I next dissect the choroid and uvea from the sclerotica; and, dividing the anterior part of the capsule into segments from its centre, I turn them back upon the ciliary zone. The ciliary processes then appear, covered with their pigment, and perfectly distinct both from the capsule and from the uvea; (Plate XV. Fig. 54.) and the surface of the capsule is seen shining, and evidently natural, close to the base of these substances. I do not deny that the separation between the uvea and the processes, extends somewhat further back than the separation between the processes and the capsule; but the difference is inconsiderable, and, in the calf, does not amount to above half the length of the detached part. The appearance of the processes is wholly irreconcilable with muscularity; and their being considered as muscles attached to the capsule, is therefore doubly inadmissible. Their lateral union with the capsule, commences at the base of their posterior smooth surface, and is continued nearly to the point where they are more intimately united with the termination of the uvea; so that, however this portion of the base of the processes were disposed to contract, it would be much too short to produce any sensible effect. What their use may be, cannot easily be determined: if it were necessary to have any peculiar organs for secretion, we might call them glands, for the percolation of the aqueous humour; but there is no reason to think them requisite for this purpose.

The *marfupium nigrum* of birds, and the horse-shoe-like appearance of the choroid of fishes, are two substances which have sometimes, with equal injustice, been termed muscular. All the apparent fibres of the *marfupium nigrum* are, as Haller had very truly asserted, merely duplicatures of a membrane, which, when its ends are cut off, may easily be unfolded under the microscope, with the assistance of a fine hair pencil, so as to leave no longer any suspicion of a muscular texture. The experiment related by Mr. Home*, can scarcely be deemed a very strong argument for attributing to this substance a faculty which its appearance so little authorises us to expect in it. The red substance in the choroid of fishes, (Plate XV. Fig. 55.) is more capable of deceiving the observer; its colour gives it some little pretension, and I began to examine it with a prepossession in favour of its muscular nature. But, when we recollect the general colour of the muscles of fishes, the consideration of its redness will no longer have any weight. Stripped of the membrane which loosely covers its internal surface, (Fig. 56.) it seems to have transverse divisions, somewhat resembling those of muscles, and to terminate in a manner somewhat similar;

* Phil. Trans. for 1796, p. 18.

(Fig. 37.) but, when viewed in a microscope, the transverse divisions appear to be cracks, and the whole mass is evidently of a uniform texture, without the least fibrous appearance; and, if a particle of any kind of muscle is compared with it, the contrast becomes very striking. Besides, it is fixed down, throughout its extent, to the posterior lamina of the choroid; and has no attachment capable of directing its effect; to say nothing of the difficulty of conceiving what that effect could be. Its use must remain, in common with that of many other parts of the animal frame, entirely concealed from our curiosity.

The bony scales of the eyes of birds, which were long ago described in the Philosophical Transactions by Mr. Ranby *, and by Mr. Warren †, afterwards in two excellent Memoirs of M. Petit on the eye of the turkey and of the owl ‡, and lately by Mr. Pierce Smith §, and Mr. Home ¶, can, on any supposition, have but little concern in the accommodation of the eye to different distances: they rather seem to be necessary for the protection of that organ, large and prominent as it is, and unsupported by any strength in the orbit, against the various accidents to which the mode of life and rapid motion of those animals must expose it; and they are much less liable to fracture than an entire bony ring of the same thickness would have been. The marsupium nigrum appears to be intended to assist in giving strength to the eye, to prevent any change in the place of the lens by external force: it is so situated as to intercept but little light, and that little is principally what would have fallen on the insertion of the optic nerve; and it seems to be too firmly tied to the lens, even to admit any considerable elongation of the axis of the eye, although it certainly would not impede a protrusion of the cornea.

With respect to the eyes of insects, an observation of Poupard deserves to be repeated here. He remarks, that the eye of the libellula is hollow; that it communicates with an air-vessel placed longitudinally in the trunk of the body; and that it is capable of being inflated from this body: he supposes that the insect is provided with this apparatus, in order for the accommodation of its eye to the perception of objects at different distances **. I have not yet had an opportunity of examining the eye of the libellula; but there is no difficulty in supposing that the means of producing the change of the refractive powers of the eye, may be, in different classes of animals, as diversified as their habits, and the general conformation of their organs.

I beg leave to correct here an observation in my former paper, relative to the faint lateral radiations, which I supposed to proceed from the margin of the iris ††. I find, on further examination, that they are occasioned by reflections from the eye-lashes.

XII. I shall now finally recapitulate the principal objects and results of the investigation which I have taken the liberty of detailing so fully to the Royal Society. First, the deter-

* Phil. Transf. Vol. XXXIII. p. 223. Abr. Vol. VII. p. 435.

† Phil. Transf. Vol. XXXIV. p. 113. Abr. Vol. VII. p. 437.

‡ Mém. de l'Acad. 1735, p. 163. 1736, p. 166. Ed. Amst.

§ Phil. Transf. for 1795, p. 263.

¶ Phil. Transf. for 1796, p. 14.

** Phil. Transf. Vol. XXII. p. 673. Abr. II. p. 762.

†† Phil. Transf. for 1793, p. 178.

mination of the refractive power of a variable medium, and its application to the constitution of the crystalline lens. Secondly, the construction of an instrument for ascertaining, upon inspection, the exact focal distance of every eye; and the remedy for its imperfections. Thirdly, to shew the accurate adjustment of every part of the eye, for seeing with distinctness the greatest possible extent of objects at the same instant. Fourthly, to measure the collective dispersion of coloured rays in the eye. Fifthly, by immersing the eye in water, to demonstrate that its accommodation does not depend on any change in the curvature of the cornea. Sixthly, by confining the eye at the extremities of its axis, to prove that no material alteration of its length can take place. Seventhly, to examine what inference can be drawn from the experiments hitherto made on persons deprived of the lens; to pursue the inquiry, on the principles suggested by Dr. Porterfield; and to confirm his opinion of the utter inability of such persons to change the refractive state of the organ. Eighthly, to deduce, from the aberration of the lateral rays, a decisive argument in favour of a change in the figure of the crystalline; to ascertain, from the quantity of this aberration, the form into which the lens appears to be thrown in my own eye, and the mode by which the change must be produced in that of every other person. And I flatter myself, that I shall not be deemed too precipitate, in denominating this series of experiments satisfactorily demonstrative.

EXPLANATION OF THE FIGURES.

Plate XIII. Fig. 1. See page 256. Prop. III.

Fig. 2. See page 256. Prop. IV.

Fig. 3. See page 258. Prop. V.

Fig. 4—6. Relating to the optometer. See page 259.

Plate XIV. Fig. 7. The form of the ends of the optometer, when made of card. The apertures in the shoulders are for holding a lens: the square ends turn under, and are fastened together.

Fig. 8. The scale of the optometer. The middle line is divided, from the lower end, into inches. The next column shews the number of a concave lens requisite for a short-sighted eye; by looking through the slider and observing the number opposite to which the intersection appears when most remote. By observing the place of apparent intersection when nearest, the number requisite will be found in the other column, provided that the eye have the average power of accommodation. At the other end, the middle line is graduated for extending the scale of inches by means of a lens four inches in focus; the negative numbers implying that such rays as proceed from them are made to converge towards a point on the other side of the lens. The other column shews the focal length of convex glasses required by those eyes to which the intersection appears, when nearest, opposite the respective places of the numbers.

Fig. 9. A side view of the optometer, half its size.

Fig. 10. The appearance of the lines through the slider.

Fig.

Fig. 11. Method of measuring the magnitude of an image on the retina. See page 293.

Fig. 12. Diagonal scale drawn on a looking-glass.

Fig. 13. The method of applying a lens with water to the cornea.

Fig. 14. The appearance of a spectrum occasioned by pressure; and the inflection of straight lines seen within the limits of the spectrum.

Fig. 15. An illustration of the enlargement of the image, which would be the consequence of an elongation of the eye: the images of the candles which, in one instance, fall on the insertion of the nerve, falling, in the other instance, beyond it.

Plate XV. Fig. 16. The successive forms of the image of a large distant object, as it would be delineated by each refractive surface in the eye; to shew how that form at last coincides with the retina. E G is the distance between the foci of horizontal and vertical rays in my eye.

Plate XV. Fig. 17. Vertical section of my right eye, seen from without; twice the natural size.

Fig. 18. Horizontal section, seen from above.

Fig. 19. Front view of my left eye when the pupil is contracted; of the natural size.

Fig. 20. The same view when the pupil is dilated.

Fig. 21. Outline of the eye and its straight muscles when at rest.

Fig. 22. Change of figure which would be the consequence of the action of those muscles upon the eye, and upon the adipose substance behind it.

Fig. 23. Scale of the small optometer.

Fig. 24. Appearance of four images of a line seen by my eye when its focus is shortest.

Fig. 25. Outline of the lens when relaxed; from a comparison of M. Petit's measures with the phenomena of my own eye, and on the supposition that it is found in a relaxed state after death.

Fig. 26. Outline of the lens sufficiently changed to produce the shortest focal distance.

Fig. 27. Apparatus for ascertaining the focal length of the lens in water.

Plate XV. Fig. 28. Various forms of the image depicted by a cylindrical pencil of rays obliquely refracted by a spherical surface, when received on planes at distances progressively greater.

Fig. 29. Image of a minute lucid object held very near to my eye.

Fig. 30. The same appearance when the eye has been rubbed.

Fig. 31—37. Different forms of the image of a lucid point at greater and greater distances; the most perfect focus being like Fig. 33, but much smaller.

Fig. 38. Image of a very remote point seen by my right eye.

Fig. 39. Image of a remote point seen by my left eye; being more obtuse at one end, probably from a less obliquity of the posterior surface of the crystalline lens.

Fig. 40. Combination of two figures similar to the fifth variety of Fig. 28; to imitate Fig. 38.

Fig. 41. Appearance of a distant lucid point when the eye is adapted to a very near object.

Fig.

Fig. 42, 44. Shadow of parallel wires in the image of a distant point, when the eye is relaxed.

Fig. 43, 45. The same shadows rendered curved by a change in the figure of the crystalline lens.

Fig. 46. The order of the fibres of the human crystalline.

Fig. 47. The division of the nerves at the ciliary zone; the sclerotica being removed. One of the nerves of the uvea is seen passing forwards and subdividing. From the calf.

Fig. 48. Ramifications from the margin of the crystalline lens.

Fig. 49. The zone of the crystalline faintly seen through the capsule.

Fig. 50. The zone raised from its situation, with the ramifications passing through it into the lens.

Fig. 51. The zone of the crystalline detached.

Plate XV. Fig. 52. The crenated zone, and the globules regularly arranged on the crystalline of the partridge.

Fig. 53. The order of the fibres in the lens of birds and fishes.

Fig. 54. The segments of the capsule of the crystalline turned back, to shew the detached ciliary processes. From the calf.

Fig. 55. Part of the choroid of the cod-fish, with its red substance. The central artery hangs loose from the insertion of the nerve.

Fig. 56. The membrane covering this substance internally, raised by the blow-pipe.

Fig. 57. The appearance of the red substance, after the removal of the membrane.

IV.

Experiments on the Chemical Production and Agency of Electricity. By WILLIAM HYDE WOLLASTON, M. D. F. R. S.*

NOTWITHSTANDING the power of Mr. Volta's electric pile is now known to be proportional to the disposition of one of the metals to be oxidated by the fluid interposed, a doubt has been entertained by many persons; whether this power arises from the chemical action of the fluid on the metal, or, on the contrary, whether the oxidation itself may not be occasioned by electricity, set in motion by the contact of metals that have different conducting powers.

That the oxidation of the metal is the primary cause of the electric phenomena observed, is, I think, to be inferred from the following experiments, which exhibit the galvanic process reduced to its most simple state.

Experiment 1. If a piece of zinc and a piece of silver have each one extremity immersed in the same vessel, containing sulphuric or muriatic acid diluted with a large quantity of

* Phil. Transf. for 1801, p. 427.

water, the zinc is dissolved, and yields hydrogen gas, by decomposition of the water: the silver, not being acted upon has no power of decomposing water; but, whenever the zinc and silver are made to touch, or any metallic communication is made between them, hydrogen gas is also formed at the surface of the silver.

Any other metal beside zinc, which by assistance of the acid employed is capable of decomposing water, will succeed equally, if the other wire consists of a metal on which the acid has no effect.

Experiment 2. If zinc, iron or copper, are employed with gold in dilute nitric acid, nitrous gas is formed; in the same manner, and under the same circumstances, as the hydrogen gas in the former experiment.

Experiment 3. Experiments analogous to the former, and equally simple, may also be made with many metallic solutions. If, for instance, the solution contains copper, it will be precipitated by a piece of iron, and appear on its surface. Upon silver merely immersed in the same solution, no such effect is produced; but as soon as the two metals are brought into contact, the silver receives a coating of copper.

In the explanation of these experiments, it is necessary to advert to a point established by means of the electric pile.

We know that when water is placed in a circuit of conductors of electricity, between the two extremities of a pile, if the power is sufficient to oxidate one of the wires of communication, the wire connected with the opposite extremity affords hydrogen gas.

Since the extrication of hydrogen, in this instance, is seen to depend on electricity, it is probable, that in other instances, electricity may be also requisite for its conversion into gas. It would appear, therefore, that in the solution of a metal, electricity is evolved during the action of the acid upon it; and that the formation of hydrogen gas, even in that case depends on a transition of electricity between the fluid and the metal.

We see, moreover, in the first experiment, that the zinc, without contact of any other metal, has the power of decomposing water; and we can have no reason to suppose that the contact of the silver produces any new power, but that it serves merely as a conductor of electricity, and thereby occasions the formation of hydrogen gas.

In the third experiment also, the iron by itself has the power of precipitating copper, by means, I presume, of electricity evolved during its solution; and here likewise the silver, by conducting that electricity, acquires the power of precipitating the copper in its metallic state.

The explanation here given receives additional confirmation from comparative experiments which I have made with common electricity; for it will be seen, that the same transfer of chemical power, and the same apparent reversion of the usual order of chemical affinities in the precipitation of copper by silver, may be effected by a common electrical machine.

The machine with which the following experiments were conducted, consists of a cylinder seven inches in diameter, with a conductor on each side, 16 inches long, and $3\frac{1}{2}$ inches diameter,

diameter, each furnished with a sliding electrometer, to regulate the strength of the spark received from them.

Experiment 4. Having a wire of fine silver $\frac{1}{120}$ of an inch in diameter, I coated the middle of it for two or three inches, with sealing wax, and by cutting through in the middle of the wax, exposed a section of the wire. The two coated extremities of the wire thus divided, were immersed in a solution of fulphate of copper, placed in an electric circuit between the two conductors; and sparks, taken at $\frac{1}{10}$ of an inch distance, were passed by means of them through the solution. After 100 turns of the machine, the wire which communicated with (what is called) the negative conductor, had a precipitate formed on its surface, which, upon being burnished, was evidently copper; but the opposite wire had no such coating.

Upon reversing the direction of the current of electricity, the order of the phenomena was of course reversed; the copper being shortly re-dissolved by assistance of the oxidating power of positive electricity, and a similar precipitate formed on the opposite wire.

Experiment 5. A similar experiment made with gold wires $\frac{1}{100}$ of an inch diameter, in a solution of corrosive sublimate, had the same success.

The chemical agency, therefore, of common electricity, is thus proved to be the same with the power excited by chemical means; but, since a difference has been observed in the comparative facility with which the pile of Volta decomposes water, and produces other effects of oxidation and de-oxidation of bodies exposed to its action, I have been at some pains to remove this difficulty, and can at least produce a very close imitation of the galvanic phenomena, by common electricity.

It has been thought necessary to employ powerful machines, and large Leyden jars, for the decomposition of water; but when I considered that the decomposition must depend on duly proportioning the strength of the charge of electricity to the quantity of water, and that, the quantity exposed to its action at the surface of communication depends on the extent of that surface, I hoped that, by reducing the surface of communication, the decomposition of water might be effected by smaller machines, and with less powerful excitation, than have hitherto been used for that purpose; and, in this hope, I have not been disappointed.

Experiment 6. Having procured a small wire of fine gold, and given it as fine a point as I could, I inserted it into a capillary glass tube; and after heating the tube, so as to make it adhere to the point and cover it in every part, I gradually ground it down, till, with a pocket lens, I could discern that the point of the gold was exposed.

The success of this method exceeding my expectations, I coated several wires in the same manner, and found, that when sparks from the conductors before-mentioned were made to pass through water, by means of a point so guarded, a spark passing to the distance of $\frac{1}{8}$ of an inch would decompose water, when the point exposed did not exceed $\frac{1}{700}$ of an inch in diameter. With another point, which I estimated at $\frac{1}{1500}$, a succession of sparks $\frac{1}{20}$ of an inch in length, afforded a current of small bubbles of air.

I have since found, that the same apparatus will decompose water, with a wire $\frac{1}{16}$ of an inch diameter, coated in the manner before described, if the spark from the prime conductor passes to the distance of $\frac{1}{16}$ of an inch of air.

Experiment 7. In order to try how far the strength of the electric spark might be reduced by proportional diminution of the extremity of the wire, I passed a solution of gold in *aqua regia* through a capillary tube, and, by heating the tube, expelled the acid. There remained a thin film of gold, lining the inner surface of the tube, which, by melting the tube, was converted into a very fine thread of gold, through the substance of the glass.

When the extremity of this thread was made the medium of communication through water, I found that the mere current of electricity would occasion a stream of very small bubbles to rise from the extremity of the gold, although the wire, by which it communicated with the positive or negative conductor, was placed in absolute contact with them. Hence it appears, that decomposition of water may take place by common electricity, as well as by the electric pile, although no discernible sparks are produced.

The appearance of two currents of air may also be imitated, by occasioning the electricity to pass by five points of communication on both sides of the water; but, in fact, the resemblance is not complete; for, in every way in which I have tried it, I observed that each wire gave both oxygen and hydrogen gas, instead of their being formed separately, as by the electric pile.

I am inclined to attribute the difference in this respect to the greater intensity with which it is necessary to employ common electricity; for, that positive and negative electricity, so excited, have each the same chemical power as they are observed to have in the electric pile, may be ascertained by other means.

In the precipitation of copper by silver, an instance of de-oxidation (or phlogistication) by negative electricity has been mentioned; the oxidating power of positive electricity may be also proved, by its effect on vegetable blue colours.

Experiment 8. Having coloured a card with a strong infusion of litmus, I passed a current of electric sparks along it, by means of two fine gold points, touching it at the distance of an inch from each other. The effect, as in other cases, depending on the smallness of the quantity of water, was most discernible when the card was nearly dry. In this state a very few turns of the machine were sufficient to occasion a redness at the positive wire, very manifest to the naked eye. The negative wire, being afterwards placed on the same spot, soon restored it to its original blue colour.

By Mr. Volta's apparatus the same effects are produced in a much less time.

Beside the similarity which has thus been traced between the effects of electricity excited by the common machine, and those observed from the electric pile, I think it appears also probable, that they originate from the same source.

With regard to the latter, its power is now known to depend on oxidation; so also does the excitement in the former appear very much to depend on the same process; for,

Experiment 9. I have found that, by using an amalgam of silver or of platina, which are not liable to be oxidated, I could obtain no electricity. An amalgam of tin, on the contrary affords a good degree of excitement. Zinc acts still better; but the best amalgam is made with both tin and zinc, a mixture which is more easily oxidated than either metal separately.

Experiment 10. But, as a farther trial whether oxidation assists in the production of electricity, I mounted a small cylinder, with its cushion and conductor, in a vessel so contrived, that I could at pleasure change the contained air.

After trying the degree of excitement in common air, I substituted carbonic gas, and found that the excitement was immediately destroyed, but that it returned upon re-admission of atmospheric air.

In conformity to this hypothesis, we find that the metal oxidated is, in each case, in a similar state of electricity; for the cushion of the machine, by oxidation of the amalgam adhering to it, becomes negative; and in the same manner, zinc, oxidated by the accumulated power of an electric pile, or simply by action of an acid, is also negative.

This similarity in the means by which both electricity and galvanism appear to be excited, in addition to the resemblance that has been traced between their effects, shews that they are both essentially the same, and confirms an opinion that has already been advanced by others, that all the differences discoverable in the effects of the latter, may be owing to its being less intense, but produced in much larger quantity.

V.

An Account of some Galvanic Combinations, formed by the Arrangement of single Metallic Plates and Fluids, analogous to the new Galvanic Apparatus of Mr. Volta. By Mr. HUMPHRY DAVY, Lecturer on Chemistry in the Royal Institution.*

I. **A**LL the galvanic combinations analogous to the new apparatus of Mr. Volta, which have been heretofore described by experimentalists, consist (as far as my knowledge extends) of series containing at least two metallic substances, or one metal and charcoal, and a stratum of fluid. And it has been generally supposed, that their agencies are, in some measure, connected with the different powers of the metals to conduct electricity. But I have found that an accumulation of Galvanic influence, exactly similar to the accumulation in the common pile, may be produced by the arrangement of single metallic plates, or arcs, with different strata of fluids.

The train of reasoning which led to the discovery of this fact, was produced by the observation of some phenomena relating to the connection of chemical changes with the evolution of Galvanic power.

* Philosophical Transactions, 1801.

It appeared, in several experiments, that series of double metallic plates, incapable of acting as Galvanic combinations, when arranged in the proper order, with portions of water, were readily made to produce Galvanic effects; by being alternated with acids, or other fluids capable of oxidating one only of the metals of the series. Thus, double plates, composed of silver and gold, (metals which have been supposed to differ very little in their powers of conducting electricity,) produced Galvanic action, when placed in contact, in the common order, with cloths moistened in diluted nitric acid. And copper and silver acted powerfully with nitrate of mercury.

These facts induced me to suppose, that the alternation of two metallic bodies with fluids, was essential to the production of accumulated Galvanic influence, only so far as it furnished two conducting surfaces of different degrees of oxidability; and that this production would take place, if single metallic plates could be connected together by different fluids, in such a manner that one of their surfaces only should undergo oxidation, the arrangement being regular.

On this supposition, I made a number of experiments on different arrangements of single metals and fluids; and, after many various processes, I was enabled to ascertain, that many of these arrangements could be made active, not only when oxidations, but likewise when other chemical changes were going on in some of their parts.

In describing the different Galvanic combinations formed by single metallic plates and fluids, I shall divide them into three classes, following, in the arrangement, the order of time with regard to discovery.

II. The first and most feeble class is composed, whenever single metallic plates, or arcs, are arranged in such a manner that two of their surfaces, or ends opposite to each other, are in contact with different fluids, one capable, and the other incapable, of oxidating the metal. In this case, if the series are numerous, and in regular alternation, Galvanic influence will be accumulated, analogous, in all its effects, to the influence of the common pile.

Tin, zinc, and some other easily oxidable metals, act most powerfully in this class of combinations.

If pieces of polished tin, about an inch square, and $\frac{1}{16}$ of an inch thick, be connected with woollen cloths of the same size, (moistened, some in water, and some in diluted nitrous acid,) in the following order, tin, acid, water, and so on, till twenty series are put together, a feeble Galvanic battery will be formed, capable of acting weakly on the organs of sense, and of slowly producing the common appearances in water; the wire from the oxidating surface of the plates evolving hydrogen; and the wire from the non-oxidating surface (when of silver) depositing oxide.

In all cases, when the batteries of the first class are erected perpendicularly, the cloth moistened in acid must be placed under the cloth moistened in water; and, in this arrangement, as the acid is specifically heavier than water, little or no mixture of the fluids will take place.

When

When zinc is employed, on account of its rapid oxidation in water containing atmospheric air, three cloths should be used; the first moistened in weak solution of sulphuret of pot-ash, (which is possessed of no power of action upon zinc, and which prevents it from acting upon the water;) the second moistened in a solution of sulphate of pot-ash, of greater specific gravity than the solution of sulphuret; and the third wetted in an oxidating fluid specifically heavier than either of the solutions. In this case, if the order be as follows, zinc, oxidating solution, solution of sulphate of pot-ash, solution of sulphuret of pot-ash, very little mixture of the fluids, or chemical action between them, will take place: and an alternation of twelve series of this kind, forms a battery capable of producing sensible effects.

III. The second class of GALVANIC combinations with single plates is formed, when plates, or arcs, composed of a metallic substance capable of acting upon sulphurated hydrogen, or upon sulphurets dissolved in water, are formed into series, with portions of a solution of sulphuret of pot-ash, and water, in such a manner that one side of every plate, or arc, is in contact with water, whilst the opposite side is acted on by the solution of sulphuret. Under these circumstances, when the alternation is regular, and the number of series sufficiently great, Galvanic power is evolved; and water, placed in the circuit with silver wires, is acted on; oxide being deposited on the wire connected with the side of the plate undergoing chemical alteration, whilst hydrogen is evolved from the side in contact with water.

Silver, copper, and lead, are each capable of forming this combination. Plates made from either of those metals, may be arranged with cloths, (moistened, some in water, and others in solution of sulphuret of pot-ash,) in the following order, metal, cloth moistened in sulphuret of pot-ash, cloth moistened in water, and so on.

Eight series will produce sensible effects; and the wire from the top of the pile produces oxide.

Copper is more active, in this class of batteries, than silver: and silver more active than lead.

IV. The third and most powerful class of Galvanic batteries, constructed with fluids and single metals, is formed, when metallic substances oxidable in acids, and capable of acting on solutions of sulphurets, are connected, as plates, with oxidating fluids and solutions of sulphuret of pot-ash, in such a manner that the opposite sides of every plate may be undergoing different chemical changes; the mode of alternation being regular.

The same metals that act in the second class, may be used in the third class; and the order of their powers is similar. The pile may be erected in the same manner as the pile with zinc in the first class; the cloths moistened in acid being separated from those moistened in solution of sulphuret, by a third cloth, soaked in solution of sulphate of pot-ash.

Three plates of copper, or silver, arranged in this manner, in the just order, produce sensible effects; and twelve or thirteen series are capable of giving weak shocks, and of rapidly producing gas and oxide in water; the wire connected with the oxidating end of the

the apparatus evolving hydrogen ; and the wire attached to the end acting on the sulphuret, depositing oxide when composed of silver, and generating oxygen when of gold.

V. In all the single metallic piles constructed with cloths, the action is very transient: the decomposition of the acids, and of the sulphurets, is generally completed in a few minutes; and, in consequence, the Galvanic influence ceases to be evolved. The arrangement of all the different series may, however, (by means of an apparatus constructed after the ideas of Count Rumford,) be made in such a manner as to give considerable permanency to their effects. This apparatus is a box, covered with cement incapable of conducting electricity, and composed of three pieces of mahogany, each containing grooves capable of receiving the edges of the different plates proper for composing the series. One half of these plates must be composed of horn, or glass, and the other half of metallic substances; and the conductors of electricity, and the non-conductors, must be alternately cemented into the grooves, so as to form water-tight cells.

When the apparatus is used, these cells are filled, in the Galvanic order, with different solutions, according to the class of the combination; and connected in pairs with each other, by slips of moistened cloth, carried over the non-conducting plates.

A combination of fifty copper-plates, arranged in this manner, with weak solutions of nitrous acid, or nitrate of ammoniac, and sulphuret of pot-ash, gives pretty strong shocks, rapidly evolves gas from water, and affects the condensing electrometer.

It does not lose its power of action for many hours; and, when this power is lost, it may be restored by the addition of small quantities of concentrated solutions of the proper chemical agents to the fluids in the different cells.

From two experiments made on copper and silver, it would appear, that the single metallic batteries act equally well, when the metals made use of are slightly alloyed, and when they are in a state of purity.

VI.

Observations and Experiments undertaken with a View to determine the Quantity of Sulphur contained in Sulphuric Acid; and of this latter contained in Sulphates in general. By RICHARD CHENEVIX, Esq. F. R. S. and M. R. I. A.*

IN a paper which I had the honour to present to the Royal Society of London, and the subject of which was the analysis of some arseniates of copper, and of iron, I had occasion, in examining many pyrites, matrices of those ores, to remark the very great inequality which prevailed in the results of repeated experiments, made with a view to determine the proportion of sulphur. But I soon perceived, that the inaccuracy was caused by a partial combustion and acidification of the radical, through the means of the nitric acid, employed to dissolve the ore.

* Irish Academy, 1801.

Having therefore, in the usual manner, ascertained what quantity of that ingredient remained untouched, I was forced to seek the rest of it in the liquor, which had washed the various precipitates. To obtain it, I poured a solution of nitrate of barytes into those washings, when all the other substances had been carefully separated, and was thereby enabled to precipitate, in a state of purity, the sulphate of barytes, formed by that earth, and by the portion of sulphur originally acidified, in the first treatment of the ore by nitric acid. To come at the knowledge of the proportion of sulphur, contained in a given quantity of sulphate of barytes, I had recourse, in the first instance, to the quantity of sulphur, said by Lavoisier to be contained in sulphuric acid, and, in the next, to the proportions of the latter, announced in the synoptic tables of Fourcroy, as entering into the composition of sulphate of barytes. According to the former of those chymists, 100 parts of sulphuric acid contain 71 of sulphur and 29 of oxygen; and again, in adopting the proportions of the latter, we have 33 per cent. of acid, on sulphate of barytes. But if 100 contain 71 of sulphur, 33 must contain 23.43. Consequently, for every 100 parts of sulphate of barytes, I was to allow 23.43 of sulphur. But, by the results of my analyses, I had such quantities of sulphate of barytes, as induced me to doubt the accuracy of one or other of the statements, by which I estimated the quantity of real sulphur contained in the ore.

No person is better acquainted than our celebrated President, with the many difficulties that occur in the analysis of salts in general; particularly with regard to the quantity of real acid they may contain. It has been a work of trouble to the ablest chymists, and they have not always agreed in their results. The proportions, announced by Fourcroy, may therefore be doubted, in common with those of the other learned operators, to whom I have alluded.

The real quantity of acid, produced by the combustion of any acidifiable basis, can be determined by one or other of the following methods only: by direct combination, in some salt, the proportions of which are already known; or by obtaining, in a state perfectly free from water, the acid resulting from such combustion. To the former method, the general objections against all analyses of salts must apply. The latter is still more defective. It is by no means certain, that we have ever yet obtained any acid, in a state of perfect fixity, unless we except the phosphoric and the arsenic; for even the crystallized vegetable acids retain a portion of water in their crystallization. It is not that I absolutely deny our having obtained them so; but I say merely, that we have no proof. It would indeed be setting narrow bounds to the perfection of nature, to assert, that no combustible body could, when saturated with oxygen, assume of itself, the state of liquidity; or that the oxide of the particular substance, called hidrogen, must be present to confer that property. Doubtless, sulphuric acid may, as well as water, contain, in itself, so just a proportion of specific heat, as to remain liquid at the temperature of our globe, and under the pressure of our atmosphere. But, both water and sulphuric acid being easily volatilized, and having a powerful affinity for each other, it is not easy, if even possible, by distillation, to separate them with sufficient accuracy, in experiments of delicate inquiry. A second source

of error, therefore, remained open in this branch of the calculation, which gave the proportion of 23.43 of sulphur, as that contained in 100 parts of sulphate of barytes. However, before I would allow myself to call in question such authorities as those I have quoted, I instituted the following experiments: in a tubulated glass retort, I put 100 parts of purified sulphur, and poured upon them strong nitric acid. A quilled receiver, plunging into a Woulfe's apparatus, was adapted to the retort; and, all being well luted, I proceeded to distil. The liquor, which came over, was poured back several times upon the sulphur, until the whole was dissolved. The water, which had come over, and that, through which the nitrous gas, produced during the operation, had passed, were essayed for sulphureous acid, and no traces of it could be found. No sulphur had been volatilized; therefore no suspicion could remain, that all was not converted into sulphuric acid. The liquors, which were in the various parts of the apparatus, were united; and to them was added a sufficient quantity of nitrate of barytes. The whole was evaporated gently; because, though I am well acquainted with the very little solubility of sulphate of barytes, I well know that nitric acid will retain a small portion of it, particularly when formed in a liquor where that acid abounds. In a first experiment, I obtained 694 from 100 of sulphur; in a second, 348 from 50; and in a third, 347 from the same quantity. But the simple rule of three reduced these quantities to 14.6, or 14.4 per cent. of sulphur, contained in sulphate of barytes; a difference wholly to be neglected. If, therefore we take 14.5 as the average, for the quantity of sulphur contained in 100 parts of sulphate of barytes, we shall not be far from the truth. From the accordance of these experiments, repeated and varied, I had now no doubt, but concerning the source where I was to seek the error, which gave 23.43 as the just proportion.

To ascertain this point, I operated in the following manner: I prepared some lime, as pure, I believe, as chymical means can procure it. I digested white marble in muriatic acid; and, by leaving an excess of the earth, was certain, that by the superior affinity of lime for that acid, nothing else had been taken up. Upon trying the solution with ammonia, no precipitate took place. By means of carbonate of pot-ash, I separated the lime in the state of carbonate; and, after well washing the precipitate, exposed it in a platina-crucible to a violent heat, till the weight no longer diminished. I am acquainted with no more efficacious method to prepare lime, fit for the delicate purposes of scientific chymistry.

One hundred parts of this lime were dissolved in dilute muriatic acid, in the same platina-crucible, previously weighed; and then sulphuric acid was added in sufficient quantity. Sulphate of lime was precipitated; and the vessel was exposed to a heat, at first gentle, to evaporate the liquor; and then, by degrees, raised to a temperature, which could expel every thing but the combined sulphuric acid, and leave the sulphate of lime completely calcined. The crucible with the salt was then weighed, and the augmentation was 76.—It appears to me, that if we admit (and I see no reason that we should not admit it) that calcined lime and calcined sulphate of lime are wholly exempt from water, it must be clear, that the 76 additional weight were sulphuric acid; and, that the sulphuric acid must
in

in this state, more than in any other, approach nearer to what may be termed, absolutely real acid. One hundred parts of calcined fulphate of lime contain therefore

Lime	57
Sulphuric acid	43
	100

By the former experiments (those made upon sulphur converted into acid; and then united to barytes) we had the quantity of sulphur, contained in fulphate of barytes. By the latter (those made by directly combining lime with sulphuric acid) we had the proportion of real acid, contained in calcined fulphate of lime. Consequently, by knowing the ratio, that fulphate of barytes bears to fulphate of lime, with regard to the acid in each, we shall arrive at the knowledge of the quantity of sulphur contained in real sulphuric acid. For this purpose, I attempted to dissolve in water, 100 parts of fulphate of lime. But finding, in this method of proceeding, a considerable inconvenience arising from the great quantity of liquor, necessary to effect the solution of that salt, I had recourse to the following expedient. Upon 100 grains of calcined fulphate of lime, I poured some oxalic acid, which attracts the basis with an affinity superior to that exercised by sulphuric acid. Oxalate of lime was here formed; but oxalate of lime is soluble in a very small excess of any acid. A little muriatic acid operated a complete solution; and thus a great quantity of fulphate of lime required but little water to dissolve it. Into this liquor, muriate of barytes was poured, and suffered to remain some time, gently heated. By these means, any oxalate of barytes, that might have been formed, was retained in solution by the original excess of acid; and the intire quantity of fulphate of barytes was deposited. Of the exactness of all these methods which I used, as the instruments by which I ascertained these results, I convinced myself by various preliminary experiments. After the usual filtration, washing and drying at the gentle heat of a sand bath, I obtained in one experiment 183, in another 183, and lastly, in another 180. This difference does not exceed the limits of what all persons, conversant in analytic chymistry, will allow to experiments of this nature. We may therefore take 183 as the mean proportion; consequently we shall say, that 183 of fulphate of barytes contain the same quantity of sulphuric acid, as 100 of fulphate of lime; and $183 : 43 :: 100 : 23.5$. Therefore 23.5 is the proportion of acid in 100 of fulphate of barytes. But we have before seen, that 14.5 of sulphur, acidified by nitric acid, form that portion of sulphuric acid contained in 100 of fulphate of barytes: viz. 23.5. We must now say, that $23.5 : 14.5 :: 100 : 61.5$, and the fourth term will be the proportion of sulphur = 61.5, which combined with 38.5 of oxygen will form 100 of real sulphuric acid.

In neither of the proportions, whether it be of the acid contained in the salt, or of the combustible basis contained in the acid, do I agree with the two chymists whom I have quoted. This justly excited some doubts in my mind, and led me to repeat my experiments. Nor should I yet be thoroughly satisfied, if I could not, upon other grounds,

than by supposing inaccuracy in them, account for the apparent differences. We must ever expect to see the errors of our predecessors corrected by men, much inferior in abilities; but who, by possessing more certain means, supply the want of genius and invention. At the time in which the experiments were made, that determined the proportion of 33 per cent. of sulphuric acid in sulphate of barytes, it was not known that we had never obtained any barytes pure; and that a considerable portion of carbonic acid resisted the action of every degree of heat that had been applied to carbonate of barytes. The fact was, I believe, first observed by Pelletier; but the method of avoiding the inconvenience was pointed out by Vauquelin. He decomposes nitrate of barytes by fire, and a moderate degree of heat is sufficient to expel all the acid and the water. The chymists I have mentioned, performed synthetic experiments, by combining, directly or indirectly, sulphuric acid, and such barytes as they imagined to be pure. The constant similarity of their results is sufficient to prove the accuracy of their operations; but working upon an impure substance, they must have been contented with a similarity of error.

Three causes may exist which are capable of accounting for any variation, whether in *plus* or in *minus*, that might have appeared in the experiments, by which Lavoisier determined the quantity of sulphuric acid, obtained by the combustion of sulphur in oxygen gas.

1st. A part of the sulphur may be volatilized during combustion.

2d. All the sulphur may not be converted into sulphuric acid; but part may remain in the state of sulphureous acid.

3d. In rectifying, some acid may come over along with the water; or, *vice versa*, some water remain with the acid. These considerations will excuse me for having proposed a doubt, where the authority of so great a man exists against the experiments which I have related.

The method which I had used to ascertain the quantity of sulphur in an ore, had been practised by Tassaert, (*Annales de Chimie*, No. 82: Analysis of Cobalt from Tunaberg) but he calculated according to the proportions of Lavoisier and of Fourcroy. In another paper by Thenard, (*Annales de Chimie*, No. 96.) he states the proportions of sulphuric acid, obtained by treating sulphur with nitric acid, to be

Sulphur	55.56
Oxygen	44.44

100.00

but in the extract given by Guyton in that No. of the *Annales de Chimie*, the mode of operation is not described. Calcined sulphate of barytes is estimated in the same paper to contain

Barytes	74.82
Sulphuric acid	25.18

100.00

which

which proportions are as near to what I had found as can be expected; for sulphate of barytes does not contain more than 3 per cent. of water of crystallization, and they must be deducted from the quintal.

Having determined with accuracy the proportion of acid in any insoluble sulphate, it is easy to proceed to the determination of that contained in any other sulphate. The dogmatic art or analytic chymistry, in general, cannot however expect to derive such advantages from the knowledge of soluble salts, as of those, which, from their insolubility, may be used with accuracy in delicate experiments to determine the proportions of the constituent parts of bodies. But if, with this mode of operating, we compare the quantities of real acid, said (in those excellent tables with which Mr. Kirwan has enriched the science) to be contained in sulphuric acid of different specific gravities, each will serve as a proof of the validity of the other; and perhaps demonstrate, that sulphuric acid, without the intervention of water, may enjoy liquidity, at the temperature and pressure which act upon our globe.

VII.

Description of an Apparatus for Distilling, in which the Effects of Absorption are prevented without admitting the external Air. In a Letter from Mr. A. S. BURKITT.

To Mr. NICHOLSON.

SIR,

I HAVE taken the liberty of inclosing a drawing of an apparatus, to prevent the absorption which so frequently happens in distilling acids, in particular the nitrous; for which I have mostly used it with the desired effect.

Should it prove original, and meet your attention, you will confer a favour by giving it a place in your valuable Journal.

I am, SIR,

Your most obedient humble Servant,

Fleet Street. Nov. 20, 1801.

A. S. BURKITT.

Plate XVII. represents the apparatus; where A is a retort, B, the usual receiver, having a neck to receive the retort, and another from which the recurved tube F proceeds to the bottom of a second receiver C; and from this last vessel a communication is made in the well known manner with the apparatus of Woulfe D. The principal singularity and advantage of my apparatus consists in the valve E, constructed by placing a plate, or plano-convex lens upon the mouth of a smaller tube, inserted by grinding within F like the valve

in North's apparatus, but with more water way; and this advantage I find in practice to be superior both in effect and cheapness to the ingenious tube of safety of Walter. That contrivance, so familiar to chemists, consists of a perpendicular tube with a small basin at top, and another reservoir or swell at bottom, in an ascending part of the same tube; and its action when duly charged with mercury is, 1. When the internal pressure increases, the mercury rises, till the whole column is supported, after which, upon any farther increase, the entire mass of mercury is suddenly blown into the upper reservoir, and gas is at once let out, till the spring is reduced nearly to that of the atmosphere; and 2. If there be absorption, the external air in all cases is very little resisted, but is suffered to enter the vessels.

In my apparatus the pressure during the transmission of elastic fluid through the valve is constant; being equal to the re-action of the liquids in the vessels C and D, and whatever other external force may be added by the well known means: and whenever by diminution of the fire, or other causes, the spring of the gases in B shall become less than the pressure of the external air, that space will become partly vacuum; but the valve will prevent any return of the fluid from C, so that the only effect will be, that the materials in A will give out their product somewhat more rapidly from the diminished re-action.

A. S. B.

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

Discovery of Comets.

IN the journal of the 8th of prairial the Notary Caigné announced, that he had 600 fr. to pay to any one who should discover a comet. On the morning of the 23d of Messidor, C. Pons, keeper of the observatory at Marseilles, discovered one, which three able astronomers of Paris also observed the same evening. The board of Longitude, being requested to adjudge the prize, considered C. Pons as having a degree of priority, to which the three astronomers of Paris assented, and the prize was awarded to him. I am soliciting the government to offer a similar prize, to accelerate the progress of our knowledge of comets, the only branch of astronomy which is not in an advanced state. It is so easy, however, to find comets, that any one, without being an astronomer, may discover them when he pleases.

LALANDE.

Mémoires d'Agriculture, &c. Memoirs of Agriculture, and Rural and Domestic Economy, published by the Agricultural Society of the Department of the Seine, and printed by Order of the Prefect of the Department. Vol. I. Paris, 8vo. 447 p. 1 Plate. Price 4 fr.

At

AT the head of this volume are the order of the Prefect for printing it, the rules of the society, and a list of its ordinary and corresponding members, with an account of the society's labour id after which are the following memoirs—Instructions for preserving Wheat from Smut, by C. C. Cadet-de-Vaux, Parmentier, Saint-Genis, and Yvart.—Report of the Experiments of C. Houdart, Junr. on preparing and economising feed—On the means of draining various lands by simple and not expensive processes, by C. Chassiron. This memoir, which has been published separately, is preceded by some observations on the general system of inland navigation in France, and followed by a comparative table of the coal-mines worked in each department, and of those which require only navigable canals and rivers to enable them to be worked. The former are fifty-one in number; the latter, sixty. Their importance to those of the French manufactures that require fuel, our mechanic arts, and our agriculture, must be obvious, at a time when we are perhaps not sufficiently aware of the want of wood, with which we are threatened, from the great devastation of the forests in most parts of the republic; a subject which has been treated of by C. Lasterie in a separate memoir in this volume.—On the precise Signification of the terms *Agriculture* and *Rural Economy*, by C. Cels.—Some Reflections on the supposed number of Sheep in France, by C. Delong. The intention of this memoir is to excite inquiry concerning this important subject.—The advantages of nurseries on estates of a certain extent, in facilitating such annual plantations as may be suited to them, by C. Villèle.—An interesting Account of the successful Amputation of the fore leg of a Cow, which had been fractured, by C. Chaumontel.—Experiments, Inquiries, and Observations on Elms, by C. Boucher; to which C. Denorgelles has added his Experiments for obtaining Alcohol from the sap of Vegetables.—On the product of different sorts of wheat that are cultivated, and the melioration of them; or the advantages that would result from inquiring what sorts of wheat yield the most bread; by C. Chancey.—On Rural Edifices, by C. Garnier-Deschênes.—On the Manner in which the Mountains in the Cevennes are fertilized, by C. Chaptal.—On the Means of rendering fit for use Wells that have been abandoned in consequence of the Mephitisation of the Soil, by C. Cadet-de-Vaux.—Observations on the working of Wines, by C. Creusé-Latouche.—On the Cultivation of the Sugar-Cane in the Caribbee Islands, and particularly of that of Otaheite, by C. Moreau-St. Mery.—On the Cultivation of the Sugar-Cane, by C. Coffigny.—On the Cinnamon-Tree of French Guiana, by C. le Blond.—The volume concludes with accounts of the lives of Cretté de Palluel, and T. Francis de Grace.

Mag. Encycl.

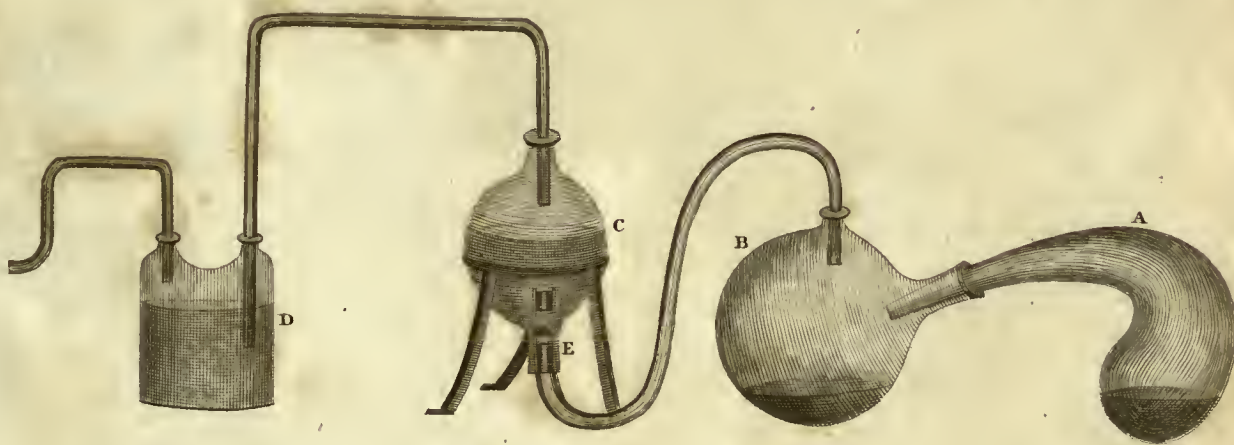
Histoire Naturelle des Mineraux, &c. The Natural History of Minerals; by E. M. L. Patrin, Associate of the National Institute; containing a Description of Minerals, and of their native Situation [Gite], the Theory of their Formation, their Connection with Geology, or the History of the Earth, an Account of their Properties and Uses, their Chemical Analysis, &c. 5 Vols. large 18mo, about 350 p. each, with 40 Plates. Price 12 fr. 50 c. plain; 18 fr. 50 c. with coloured plates; on fine paper, 5 fr. 50 c. more.

C. PATRIN

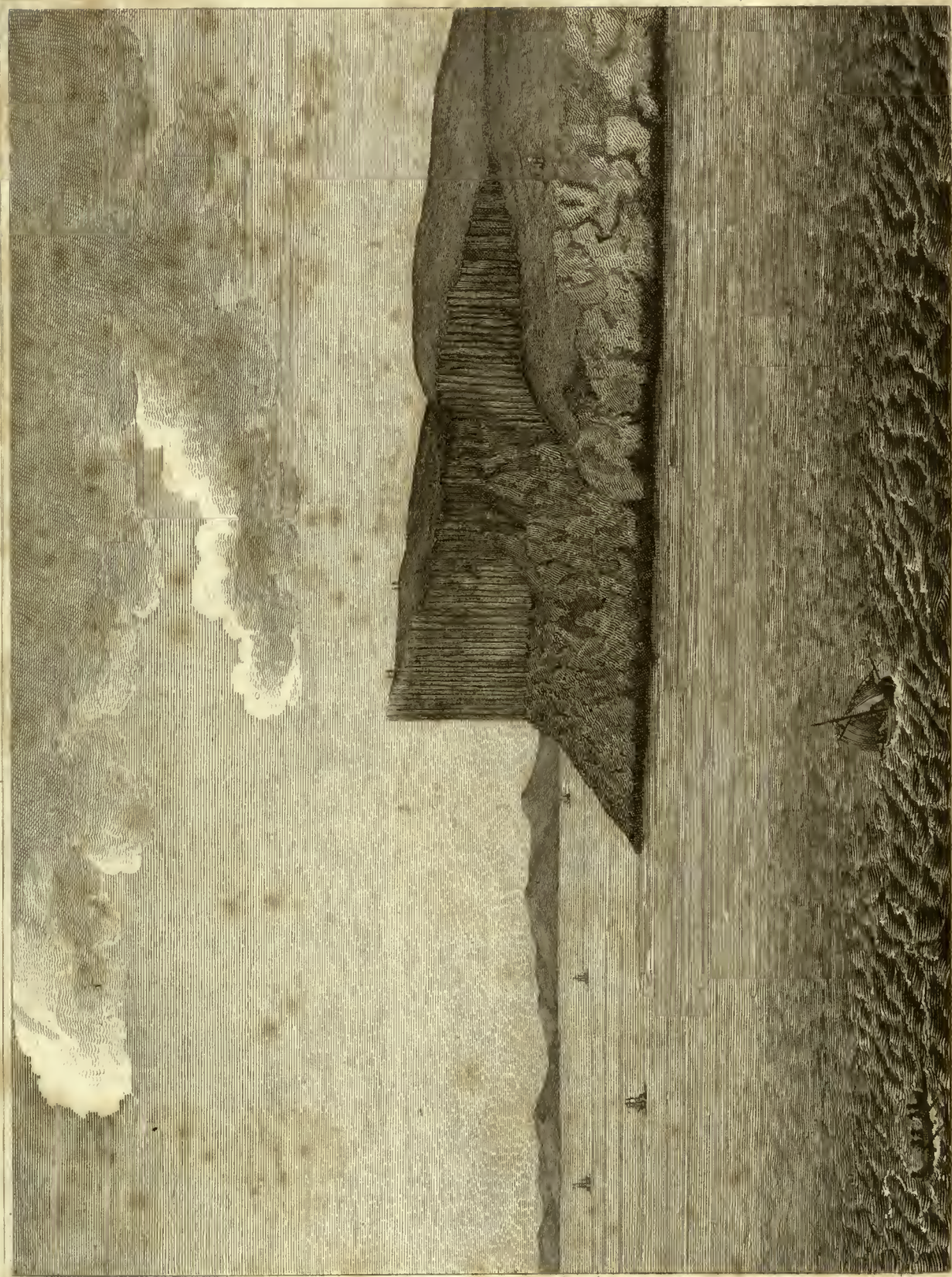
C. PATRIN has made himself known by several memoirs in the *Journal de Physique*, in 1788 and 1791, on the mines of Siberia, which he spent eight years in examining from the boundaries of Europe to the frontiers of China, and whence he has brought home a collection of minerals as valuable as it is ample. The object of this tour was to collect information concerning the structure of mountains in general, the formation of metallic veins, the relation that different mineral substances bear to each other, and the history of the terrestrial globe itself. It is particularly in this important point of view, that the author has treated of the history of minerals. This too was the object proposed by the illustrious Buffon; but that great man wanted the aid of observation, which genius cannot supply; and what he has said on the subject is built wholly on systems formed in the closet, and now considered only as splendid chimeras. To these different notions of general theory, the author has added all the particulars that he judged of most importance respecting the properties of the different mineral substances, and their uses in physic or the arts: every thing, in short, that can render them serviceable to man. He has carefully added the analyses made of them by the most celebrated modern chemists, which were altogether omitted by Buffon; though, indeed, had they not, what he could have given would have been of little use at present, from the amazing superiority the chemical operator has attained since his time. In this work is an essay on volcanoes, in which C. P. advances a new theory of these grand phenomena: and one thing shews, that the author has seen the operations of nature contemplated as they actually are; his having pointed out several discoveries as possible, which have been really made since his memoir was read at the Institute.

END OF THE FIFTH VOLUME.

*Mr. Burkill's arrangement of distillatory Vessels
to prevent the effects of absorption.*







View of the basaltic Columns of Fairhead, on the Coast of Ireland.



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$\frac{1}{5}$ Rec.



